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OCKAM'S RAZOR AND THE UNIFICATION OF PHYSICAL SCIENCE *

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1 *The Unification of Physical Science*

As understood here the unification of physical science constitutes the replacement of many specific laws, principles, and hypotheses by a smaller number of more general ones. An example of the process, which is frequently quoted and which stands out as pre-eminent, is the unification achieved by Newton.

Before his day there were no general laws of mechanics; there were only a variety of specific laws, each applicable to a specific mechanical system. It was believed that a specific law, applicable only to planets, required these to move in elliptical orbits; that a quite distinct specific law, applicable only to pendulums, required their period of swing to bear a specific relation to the length of the pendulum; that yet another specific law, applicable only to vacuous spaces, required these to be filled. If there were such a thing as a Cosmic Statute Book, this would have had to contain, according to the pre-Newtonian view, separate entries under the respective headings Planets, Pendulums, Vacuum. The book would have been a bulky one.

But Newton showed that many such specific laws were implicit in other more general ones. A large number of observed facts could be inferred from his laws of motion and gravitation. If the Cosmic Statute Book contained these all-embracing laws, there would be no need for further entries to say that planets shall move in elliptical orbits, that the period of swing of a simple pendulum shall be proportional to the square root of its length, that a projectile shall have a parabolic path, that a vacuous space shall be filled: Newton, it might be said, did much to whittle down the Cosmic Statute Book.

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* This paper is an elaboration of the preamble to Professor Kapp's Chairman's address to the Philosophy of Science Group on 14th October, 1957 (Ed.).

Another way of describing the same achievement is to say that Newton ‘ explained ’ a large number of facts. For in physics a fact is explained by showing how it can be inferred from something more general. All explanations there are steps in the direction of greater unification.

Since Newton's day the unifying process has extended into more and more branches of physical science. The first and second laws of thermo-dynamics have had great unifying power. From them much can now be inferred that would otherwise have to be attributed to specific, *ad hoc*, laws. The relation between the once quite distinct subjects of electricity and magnetism is found to be so close that they are now considered as one subject. A study of the relation between chemical reactions and thermo-dynamics, as also of that between chemical reactions and atomic structure, has led to the new branch of science called physical chemistry. This has taught us that chemical processes and properties are implicit in atomic structure. At one time it must have appeared that an entry in the Cosmic Statute Book would be necessary to say that hydrogen shall combine with oxygen and form a substance with the properties of water. But we now know that such a clause would be redundant. Physical chemists can tell us that, provided there be atomic nuclei with, respectively, one and eight positive unit charges, the rest is assured. One can infer, with the help of certain general laws, that atoms having such nuclei must combine and that the resultant compound must have the properties of water. This unification is making it possible to explain more and more chemical facts in terms of atomic structure.

A most valuable feature of unification is that it enables one to replace observation and experiment by inference and calculation. Galileo could only discover the law of the pendulum by observing pendulums ; but after Newton anyone who had never seen a pendulum could have discovered the law. He would have done so by the reasoning that is now taught in schools, where the formula is deduced from first principles. Similarly, the elliptical orbits of planets could only have been discovered before Newton's day by making careful observations of successive positions of planets. But now a person who had lived all his life under a blanket of impenetrable cloud and learnt for the first time that there was a massive sun surrounded by less massive bodies could predict that, when the cloud lifted, one would observe the less massive bodies to move in elliptical orbits. There was a time, again, when the chemical properties of substances seemed only to be discoverable by observing those substances. But this is not necessary today. Long before the element Hafnium had been found chemists did not only say that there was such an element; they also predicted its properties. The properties are implicit in the general laws that chemists have discovered and can be inferred from these laws.

It is this kind of unification that has made the rapid progress of technology possible. If every chemical substance had to have a clause in the Cosmic Statute Book defining its properties, chemists would have to make the substance and submit it to a laborious series of tests before they knew what the properties were. But the properties are implicit in general laws. If these are known, the properties follow automatically. Hence it is a commonplace of chemical research to predict the properties of a new compound before making it.

It is the same in all other branches of technology. Without a unified physics one would have to make a gun and fire it before one could know what path the projectile would take. One would have to make and test a bridge in order to discover its strength. One would have to make and test every new kind of engine before one could determine its thermal efficiency or the critical speed of its shaft. But the technologist's aim is to substitute inference for observation wherever possible. Doing this, he can predict the performance of guns, the strength of bridges, the efficiency of steam engines while they are still in the blue-print stage. In technology tests, observations, experiments, do not serve the purpose of facilitating predictions but of verifying them and of correcting errors and oversights. This can be done because what is predicted is implicit in general and known laws and principles.

By the process of unification the whole of physical science is gradually being fashioned into one complete and consistent structure of thought in which the various parts bear a logical relation to each other. Mechanics, electricity, magnetism, thermo-dynamics, chemistry, heat, light, sound, to mention only some branches, have been brought under one common roof.

During the present century we have seen two conservation principles, those respectively of energy and mass, united. Einstein has established in the general theory of relativity a connection between gravitation and space and has thereby brought space under the common roof with the rest of physics. There has, further, been the formulation of the very basic and comprehensive law according to which physical changes cannot be by indefinitely small amounts. This law forms the foundation of the quantum theory and has brought under the common roof a large number of observations that previously seemed to be isolated and each to require its own clause in the Cosmic Statute Book. Predictions of all sorts are being based every day on the principle that all physical changes are quantised.

The search for greater and ever greater unification continues, but with varying success. One of the failures is worth mentioning because it illustrates the nature of the problem. Three different types of field of force have been observed : magnetic, electrostatic, and gravitational. Something is known about how the first two are related to each other, and one commonly speaks of them jointly as the electromagnetic field. But yet they remain distinct from each other and quite distinct from the gravitational field, which has been shown by Einstein to be a region where the geometry of space-time differs in a specific way from Euclidean geometry. The difference can be expressed in a mathematical formula.

The hypothesis is near at hand that the magnetic field is also a region where the geometry of space-time differs from Euclidean geometry, though in a different way, and that the electrostatic field represents a third departure. If so, one might expect to be able to generalise Einstein's relativity equations in such a way that they would represent *any* kind of field. If that could be done, one specific value of a term in the equation would define the gravitational field only, another the magnetic field only and a third the electrostatic field only. Each field would then appear as a special case of something common to them all ; its properties could be predicted from the great sweeping law that was applicable to all fields ; magnetic, electrostatic, and gravitational fields would be brought under a common roof. The attempt to achieve this has been called the search for a unified field theory.

Assiduously though it has been conducted by a number of scientists, of whom Einstein was one, the search has so far only led to disappointment. It is impossible to say yet whether the failure is due to the inherent difficulty of the subject or because the search has taken a false hypothesis as its starting point, i.e. that all fields of force have enough in common for them to be represented in terms of the geometry of space-time. Yet, in spite of the apparent reasonableness of this assumption, it may not be true. Electrostatic and magnetic fields may be so different from gravitational ones in their nature, their effect, their cause, that they cannot be represented in any comparable terms at all. Some other hypothesis, one that has not yet been formulated or even thought of, might prove a better starting point for bringing electromagnetism and gravitation under a common roof.

Be that as it may, no attempt will be made here to succeed where Einstein and others have failed. The present study is in no way concerned with the search for a unified field theory, desirable though it is that the search should continue. But the example will help to define the scheme according to which unification in physics occurs.

How is unification achieved? A general principle is first found. Examples are Newton's laws of motion, the great conservation laws, the principle of least action, the principle according to which all observable physical changes are quantised, the principle of the equivalence of mass and energy, the principle by which the chemical properties of substances are related to the number of electrons that surround the neutral atoms of the constituent elements.

At the next step towards unification various phenomena are shown to be implicit in one or other of these principles. They can therefore be inferred from them and so could be struck off the Cosmic Statute Book as redundant.

Sometimes the phenomena are observed first and the principles are found later. The principles are then said to explain the phenomena. Thus the observed behaviour of planets was explained by the laws of motion and gravitation. Similarly, attempts to make a perpetual motion machine failed for unexplained reasons until the principle of conservation of energy provided the explanation.

At other times the principle is found first and some phenomenon that is implicit in it is described before it has ever been observed. In such instances it may, or may not, be observed later. One then says that the phenomenon is predicted by the principle. Engineers, as mentioned already, follow this course as a matter of routine. They invent and design new kinds of machines on the basis of the great sweeping principles of physics and they predict their performance. Observation and experiment come later—and not to test the principles but to test the soundness of the designer's reasoning. In physics, too, it sometimes happens that a phenomenon is predicted as an inference from a general principle before it has been observed. The properties of Hafnium have already been quoted as an example. But physicists work most often with things that they are observing at the time. Their concern, unlike that of the machine designer and the industrial chemist, is more often to explain observed effects than to predict those that will only later become observable.

The striving to bring ever more phenomena under the common roof, to unify the whole of physics is, of course, not the whole of the physicist's work. Indeed most research workers are concerned only with the discovery of the detailed facts, qualitative and quantitative, of the physical world ; and necessarily so, for we still have much to learn about the laws of mechanics, heat, light, sound, electricity and magnetism, about the physical and chemical properties of solids, liquids, and gases, about the macrostructure and the microstructure of the material universe, about the positions and movements of the heavenly bodies. But, nevertheless, it is worth stressing that much of the thinking done by most physicists is directed towards the discovery of generalisations and that it is on these as much as on collections of observed facts that physical science is based.

The distinction between the search for isolated facts and the search for unifying generalisations is well illustrated by the elliptical orbits of planets. We can now understand why these orbits could not be explained before Newton's day. It was because of a wrong outlook. During and for some time after the Middle Ages the notion was prevalent that every phenomenon was the result of what might be called a distinct act of legislation, that it was ensured by what I have metaphorically called a separate clause in a Cosmic Statute Book. Those who held this view were bound to think that one could only find out anything important about planets by studying planets ; and that it was idle to ask why they moved in ellipses. The acceptable answer was that such orbits were a legislative requirement, about which no further questions could or should be asked.

But we can now realise that, if the elliptical orbits could not be explained before Newton, it was not that they were inexplicable. Nor was it that not enough was known about planets. It was that not enough was known about mechanics. No further astronomical research, no careful observation of the orbits, no precise measurement could have provided the explanation. But Newton's laws of motion and gravitation did so. In other words, scientists only found the answers to some specific questions about planets when they had found statements that were general enough to apply to all ponderable objects. In medical metaphor, ignorance about planets proved to be, not the disease itself, but a symptom of the disease. Newton followed the course of a doctor who seeks to treat the disease rather than the symptom.

Similarly our inability until a short while past to explain why given chemical substances react in the observed ways proved to be due, not to our ignorance of the substances, but to our ignorance of the more general subject of atomic structure. The great generalisations on which modern chemistry is based could not have been discovered by work conducted only in the field of chemistry.

These considerations are relevant to the present study because I propose to demonstrate here the great explanatory power of the generalisation that is reached when one pursues the search for a unifying principle in physics with uncompromising consistency. It will be shown below that one then reaches a very comprehensive principle, one that is applied by physicists on occasion but which has not been given the status it deserves.

2 *The Principle of Minimum Assumption*

Any search for greater unification of physics must have a reasoned beginning ; it must be conducted methodically ; the unifying principle sought must conform to the requirements of scientific method. So let the starting point of this enquiry be one of the commonplaces of scientific method—the rule of economy of hypotheses, sometimes called Ockam's razor. This says that when more than one explanation of an observation is available one must provisionally choose the one that involves the least number of assumptions. The rule is so well known and so generally accepted that there is no need to illustrate it by examples. Most of us never doubt that it is a good rule, although we may differ as to how rigidly it should be applied. To assess its value we must first consider its nature and then the place that it occupies in scientific research. We shall find that in the history of physics the importance of this rule is very great indeed.

The rule of economy of hypotheses is one of the tenets of scientific method. It tells the scientist what to do when more than one explanatory hypothesis is available, but it does not guarantee that the recommended choice will be justified by events. This is evident from the use of the word ' provisionally '. By advising that the minimum assumption be made provisionally the rule allows for the possibility that another hypothesis, one that does not meet the criterion of minimum assumption, may have to replace it some day.

The great principles of physics are in a different category. They are not mere rules of procedure but statements about the very nature of the physical world. They are so well established that the word ' provisional ' is omitted from their formulation. The principle of conservation of energy is an example. It is concerned with the energy in a given system, which may be energy of motion, of position, of chemical structure, of mass. The principle asserts that, provided the system be self-contained, the total quantity of energy in it is constant. Many conclusions can be inferred from this great principle. One of them is that a perpetual motion machine is impossible. So the principle of conservation of energy suffices to refute a person who claims to have invented such a machine. It would be highly exaggerated caution to tell him that his idea has to be rejected provisionally ; one rejects it outright. One does not say that it is unlikely that the machine will work ; one says that the principle of conservation of energy proves the inventor's idea to be wrong.

It is not so with a statement that violates the rule of economy of hypotheses. Almost every week letters appear in the daily press and articles in scientific journals, papers are presented to learned societies, in which hypotheses are put forward that involve more, sometimes much more, than a minimum assumption. One may deplore such hypotheses, but it is not customary to use Ockam's razor to prove them wrong.

Here is a significant difference between a principle and a rule of procedure. One can *refute* a statement that violates the principle of conservation of energy, but one can do no more than *deprecate* a statement that violates the rule of economy of hypotheses.

In making this distinction do we give a sufficient status to the rule of economy of hypotheses ? It depends, I am venturing to suggest, on the discipline with which one is concerned. In history, in biology, in the social sciences, the rule can be no more than a useful guide to procedure ; statements that conform to it can only be accepted provisionally ; they may eventually have to be replaced by statements that violate the rule. But I wish to make the bold claim here that, in physics, the rule of economy of hypotheses can be so expressed and defined that it acquires a status far higher than the one usually accorded to it ; I wish to raise it from a mere rule of procedure to one of the great universal principles to which the whole of the physical world conforms. At this level it would be worded as follows : *In physics the minimum assumption always constitutes the true generalisation.* It needs a name so I propose to call it the *Principle of Minimum Assumption.*

This claim is, itself, a hypothesis and has to be justified. I claim that one is able to do this by showing that a unified cosmology is achieved by the consistent, uncompromising, and methodical application of the principle of minimum assumption to theories about the past and future duration of matter. But before I do this I shall have to discuss the nature and meaning of the principle and show that it is applied by scientists already and more often than is always appreciated.

What is a minimum assumption? One sometimes hears the remark ‘ that is a big assumption ’, just as one hears ‘ that is a big lie ’. The implication of such colloquial habits of speech is that qualitative distinctions can be made between different assumptions and between different lies ; that one could arrange a collection of assumptions or lies in a row, with the biggest at one end and the smallest at the other ; that the magnitude of assumptions and lies could be expressed in units, like that of temperature, hardness and other measurable quantities.

It may be so for all I know. But I am not concerned here with the grading of assumptions according to size. I am concerned instead with the search for a criterion by which an assumption that is defined as a minimum one can be clearly distinguished from assumptions that cannot be so defined. I do not think that the criterion is hard to find. I think it is whether the assumption is specific or not ; so I shall define a minimum assumption as one that is completely unspecific. What this means can best be explained with the help of some examples. The first of them will be deliberately chosen to be extreme to the point of absurdity.

A young man who has had a predominantly humanistic education has been fascinated by a popular book on astronomy and has become enthusiastic about what he calls the beauties of science. He has read in his book that most stars do not have planets but that a very small proportion of them do and that these form solar systems like our own. The total number of stars is great, he learns, and so even the tiny fraction that have solar systems amounts to millions of stars.

He has also read in earlier books that planets are sublime bodies, constrained by their noble natures to move in orbits of geometrical perfection. In doing so, he has gathered, they produce a lovely harmony, known as the music of the spheres. He reaches the not unnatural conclusion that, where perfection is displayed in terms of geometry and music, it must also be displayed in terms of number, so he arrives at the hypothesis that every one of those millions of distant solar systems must have a pleasing number of planets. He can well believe that this may be the mystical number seven, or the virile number nine, or occasionally perhaps the round number ten. But he feels sure that no solar system can be cursed with the unlucky number thirteen.

An astronomer friend reproves him for his unscientific outlook. To believe that the number thirteen is precluded is, he says, a big assumption and an unjustified one. He tells the young man that some solar systems do have thirteen planets. The young man remains puzzled. Why, he asks, is it a big and forbidden assumption to believe that no solar system has thirteen planets and a small and permitted assumption to believe that some solar systems do have thirteen planets?

The question is not a silly one. It must not be dismissed with a shrug and a smile. It is basic to scientific method and it behoves us to find a clear and direct answer to it.

The answer is not that solar systems with thirteen planets have been observed. The astronomer admits that his belief in the existence of such solar systems is a hypothesis. The resolving power of our best telescopes is not sufficient to reveal any solar system but our own. So far as observation goes we have no proof that there is any other solar system at all.

Nor is the answer that there is a law of physics by which some solar systems are required to have thirteen planets. The reason for the astronomer's belief is, on the contrary, the very *absence* of any known law to require that solar systems shall have a specific number of planets. Here the minimum assumption is the unspecific one, i.e. that *any* number of planets can occur. This is therefore the assumption that, in conformity with the demands of scientific method, is made by our astronomer. He would not feel justified in making any other.

From the above little story one may learn the operative word by which to recognise a minimum assumption. It is ' any '. It need not apply only to numbers, but can also apply to quantities, properties, relationships, configurations, to any feature that one likes to mention. When there are only two alternative possibilities the grammatical substitute for ' any ' is ' either ' as, for instance, when there is a choice between the positive and the negative sign. So a minimum assumption can be recognised by the use in its formulation of the words ' any ' or ' either '.

In practice it is not difficult to distinguish between a minimum assumption, as just defined, and one that is not a minimum one. But the question remains whether, when one has recognised a minimum assumption, one is always justified in making it. In the example of the number of planets in a solar system one cannot be sure whether the minimum assumption is the true generalisation or not. For it is impossible to prove by observation that solar systems may have *any* number of planets. But there are many occasions in the history of physics when the minimum assumption has proved to be the true generalisation. Let this be demonstrated with the help of some real examples.

3 *Use of the Principle of Minimum Assumption in Physics*

Minimum assumptions have not received as much attention as one might have expected in the philosophy of science. The distinction between specific and unspecific assumptions is not a textbook subject; and so but little has hitherto been done to clarify thought about it. But the history of science shows that specific assumptions have been made in the past time and time again; that they have been treated as generalisations about the nature of the physical world, and have eventually had to be replaced by unspecific ones. Whenever this has happened new light has been shed on a wide range of subjects; the unifying and explanatory power of the unspecific assumption has been demonstrated.

Thus it was assumed at one time that a specific law, applicable only to planets, constrained these to move in elliptical orbits. But Newton replaced the hypothesis that the planetary orbits were the consequence of a specific law by the hypothesis that they were the consequence of the circumstances in which the planets found themselves. The assumption that certain bodies are required by their natures to move in specific ways was replaced by the assumption that any body may move in any way, its actual path being determined by the forces exerted on it. This proved to be the true generalisation.

Similarly, it was assumed at one time that Nature has specific likes and dislikes; for instance, that she abhors a vacuum. This could be translated into contemporary language as the specific assumption that a law of physics prevents the density of matter from falling below a specific value. But it is now known that the true generalisation about the density of matter is unspecific. The laws of physics permit any density, ranging from the high concentration that occurs in the white dwarf stars to the extreme tenuousness of extragalactic space.

A further illustration may be taken from more recent history—the geometry of space. Until some fifty years ago it was assumed that this was required by the laws of physics to be of the kind known as Euclidean. If the assumption was not recognised as a specific one, it was only because it was not recognised as an assumption at all; it was thought of as a self-evident truth. Nevertheless, mathematicians had already shown that other geometries were logically possible. But very few persons believed that they were also physically possible.

Einstein, however, was prepared to take the same view about the geometry of space that the astronomer in our little story took about the number of planets in solar systems. Knowing of no law to preclude non-Euclidean geometries, he made the minimum assumption, namely that they can occur. The unifying and explanatory power of this assumption has proved to be enormous.

Yet another, rather simple, example is provided by the periodic table of the elements. The basic feature by which the chemical properties of an element are determined is the number of unit charges on the nuclei of its atoms. For the elements to be found in nature the maximum number of such charges is ninety-two, a stability limit. This limit provided a logical reason why no nucleus could be observed in nature with more than ninety-two unit charges, for a greater number would be inconsistent with the definition of a stable particle. But there was no logical reason why a smaller number should not occur. The minimum assumption is that a stable nucleus may carry any number of unit charges up to ninety-two.

There was a time, not so very long ago, when observation had nearly, but not quite, justified this assumption. Nearly all the numbers had been observed, but there were a few gaps. One of these was seventy-two. Another was zero.

Those who subscribed to the doctrine that a scientist must never, never believe what he cannot observe would have been precluded from believing that nuclei with seventy-two unit charges could occur. But few, if any, scientists carried their faith in empiricism thus far. Their faith in the principle of minimum assumption was the stronger, at least about the number seventy-two. So an element corresponding to this number was predicted purely on the basis of this principle. Its properties were also inferred and predicted. The event justified faith in the principle, for the time came when an element with seventy-two unit charges was observed and found to have the predicted properties. It received the name Hafnium.

Had faith in the principle of minimum assumption been a little stronger and the word ' any ' taken a little more literally, scientists would also have predicted nuclei, or at least particles, with zero charge. For they would have noticed that there was neither a law nor a logical reason why such particles should not occur. Having satisfied themselves about their logical possibility, they would next have worked out what properties would follow logically from the definition of a particle with zero charge. They would have decided that it could not attract any satellite electrons ; that it would pass through any atom without being deflected by the charges on the nucleus of the atom ; that therefore no vessel could contain it ; that it would not enter into any chemical reaction. They would, in short, have predicted the neutron together with its properties. Subsequent observation of neutrons would then have served to justify the choice of a minimum assumption.

Actually the sequence of events was reversed. The neutron was observed first and its occurrence and properties were explained afterwards. But no other hypothesis was needed for the explanation than that any particle may carry any number of unit charges, including zero. The neutron provides one of many illustrations of the great explanatory power of the principle of minimum assumption.

Let me quote one further illustration of the power of this principle. It is the discovery of the positron by Dirac. The scientific work that led him to predict it is recondite and need not be described in detail. A few salient facts, deliberately presented in an over-simplified form, will suffice to point a moral.

One of Dirac's equations had two solutions, as happens when one solves a quadratic equation. One of the terms in both these solutions represented energy ; but it occurred with the positive sign in one of them and with the negative sign in the other. The positive sign caused no difficulty. It represents energy as we know it. But the negative sign could only mean that the solution applied to a system that contained negative energy. It was difficult to give meaning to negative energy ; but this was not the only objection to the second solution. Just as, at one time, no-one had observed particles that had seventy-two or zero charges, so no-one had observed a state of negative energy.

Had Dirac been a slave to the doctrine that what is not observable has no place in reality he would have had to assume that a specific law of physics prohibits a state of negative energy. But instead of assuming this he allowed himself to be guided by the principle of minimum assumption. He saw that it would involve a specific assumption to deny the possibility of negative energy and a minimum assumption to accept that possibility. His reasoning was strictly analogous to that of the astronomer who finds it more consistent with scientific method to postulate than to deny that some solar systems have thirteen planets.

Dirac's next step was to seek possible reasons why negative energy had never been observed. He rejected the facile answer that there was no such thing. Having satisfied himself that negative energy was logically possible he was convinced that it was also physically possible. The answer that Dirac did find need not concern us, but what is relevant is that in seeking it he reached the conclusion that evidence for negative energy would be provided by a particle with the mass of an electron and positive unit charge. This conclusion did not involve any additional hypothesis or assumption ; it was a logical inference from work based on no other hypothesis than that the minimum assumption is always the true generalisation.

That the predicted particle had no more been observed than a state of negative energy did not shake Dirac's faith in the principle, a faith that was justified when, some years later, the particle was actually observed. It is called the positron.

Until this happened there were doubts about Dirac's prediction ; and it is important to appreciate their nature. It was thought that the reasoning might have been faulty ; that some essential fact might have been overlooked ; that the mathematics might have contained an undetected error. The eventual discovery of the positron served to allay doubts of this kind. But they were all doubts as to whether a state of negative energy was really logically possible. Few doubted that, if it was, it was also physically possible and would occur occasionally.

Examples where, in physics, the principle of minimum assumption has led to new and valuable discoveries, when it has served to predict, to explain, to unify could be multiplied indefinitely. When a minimum assumption has been used as a basis of the subsequent reasoning, a number of valuable conclusions have followed from it by a process of logical inference and without the need for any additional hypotheses. But I cannot recall a single instance in physics where a specific assumption has, after scrutiny, been maintained as a true generalisation. For such examples one has to turn to history, to biology, to the social sciences, to the study, in other words, of systems that come under the influence of life.

4 *The Concept of a Cosmic Statute Book*

I have introduced the expression Cosmic Statute Book above and have discussed this concept in detail elsewhere.[1] It must suffice to mention here only one or two points connected with it.

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[1] R. O. Kapp, *Science versus Materialism*, London, 1940, Chapters XXII and XXV; *Facts and Faith*, London, 1955

Laws are always concerned with generalisations. The laws that govern the formation of companies, finance acts, the rule of the road, apply to all companies, to all tax payers, to all road users within the country for which they are enacted. This holds equally for what are called the laws of physics. But there the resemblance ends.

Laws imposed by authority can be, and often are, recorded in statute books, so I shall say that they are of the statute book kind. What characterises them is that they demand a specific choice between alternatives all of which are logically possible. They require, for instance, that traffic shall keep to its own side of the road and prohibit passage on the opposite side.

In the formulation of such laws the words ' any ' and ' either ' may not occur. If they did the law would be meaningless. Further, such laws do not include what is the logical consequence of other accepted principles. If they did they would be redundant. Thus no country would enact a law to say that people may drive *either* on the right or on the left. Such a law would enforce nothing and prohibit nothing. Nor would any country enact a law to require that two twos shall be four. It would be so whether the law were enacted or not.

The question now arises whether there are laws of physics that demand a specific choice between alternatives that are logically possible. We believe that it is logically possible for a solar system to have thirteen planets, and we know that it is logically possible for a particle to have seventy-two and zero unit charges, for space to have a variety of different geometries, for energy to occur in the negative state. If, nevertheless, a law of physics prevented any of these possibilities, it would be of the statute book kind. Such a law would have no place in a unified physics. It could not be inferred from any known principle. It could not be explained. It could only be discovered by observation.

According to the principle of minimum assumption there are no such laws in physics and I have shown above with the help of a few examples that physicists often act on the belief that it is so. Their belief can be expressed by rewording the principle of minimum assumption as follows : *In physics a generalisation that is logically possible is also physically possible. It can therefore be represented by an actual example and is so represented with a frequency that is determined by statistical considerations only.*

Yet another formulation of the same principle is as follows : *For the physicist there is no such thing as a Cosmic Statute Book.*

This negative formulation brings the principle into the category of what Sir Edmund Whittaker called 'postulates of impotence'. It tells us what one cannot do. It says that one cannot base a true generalisation in physics on a specific assumption. Therewith it has a faint resemblance to a formulation of the principle of conservation of energy that says : One cannot make a perpetual motion machine. This negative wording has its advantages sometimes. In mechanics one tests conclusions for their conformity to the principle of conservation of energy and one may reject them by saying, 'That is equivalent to inventing perpetual motion'. If we got into the habit of testing our conclusions for their conformity to the principle of minimum assumption we should similarly find ourselves saying sometimes, 'That is equivalent to an entry in a Cosmic Statute Book'.

However, the principle of minimum assumption is far from having reached universal acceptance. People have not become very articulate about it. It is by no means applied with the uncompromising consistency that it needs. Those who do apply it do so more instinctively than with deliberation and many of them would oppose my plea for elevating the rule of economy of hypotheses to the status of a great principle of physics.

Nevertheless, this is exactly what, I claim, should be done. On the more superficial view many discoveries in physics might be regarded as ingenious explanations of certain observed phenomena. But what I want to emphasise is that they need not have been found as a result of a search for *ad hoc* explanations. They have a different status, that of inferences. For they could equally well have been found as a result of exploring some implications of the principle of minimum assumption.*

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* During my address I developed in detail the theme of the predictive and explanatory power of the principle of minimum assumption. I showed that one can infer from this principle, and without the need for any further hypotheses, a number of cosmic phenomena that have hitherto eluded explanation. The selected examples were the expansion of space, the occurrence and detailed structure of the spiral nebulae, and the familiar observation that every large accumulation of inertial mass is the source of a gravitational field.

ON PREDICTION AND EXPLANATION *

NICHOLAS RESCHER

ANALYSES in the philosophy of science frequently emphasise the logical similarities between prediction and explanation. It is said that prediction and explanation are identical from a logical standpoint, in that each is an instance of the use of reasoning in support of an hypothesis, and it is contended that the sole point of difference between them is that the hypothesis of a prediction concerns the future, while explanations concern the past. We read, for example, that

> the difference between the two [i.e. prediction and explanation] is of a pragmatic character. If E [the conclusion of the explanatory schema] is given, i.e. if we know that the phenomenon described by E has occurred . . . we speak of an explanation of the phenomenon in question. If . . . E is derived prior to the occurrence of the phenomenon it describes, we speak of a prediction.[1]

The sole element of difference between explanations and predictions, it is said, resides in the fact that one concerns itself with the past, the other with the future, and this material distinction alone separates two processes which, formally and logically, are otherwise identical.[2]

The present paper will attempt to show that this view of the relationship between explanation and prediction is mistaken, being wrong

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[1] P. 138 of C. G. Hempel and P. Oppenheim, ' Studies in the Logic of Explanation ', *Philosophy of Science*, 1948, **18**, 135-175. Reprinted in part in H. Feigl and M. Brodbeck, *Readings in the Philosophy of Science* (New York, 1953), pp. 319-352.

[2] Regarding this supposed parallelism of explanation and prediction see: K. Popper, *Logik der Forschung* (Vienna, 1935), pp. 26 sqq. (this work has just appeared in English translation); idem, *The Open Society and its Enemies* (London, 1945), Vol. 2, pp. 249, 342 sq.; and C. G. Hempel, ' The Function of General Laws in History ', *Journal of Philosophy*, 1942, **39**, 35-48, reprinted in H. Feigl and W. Sellars, *Readings in Philosophical Analysis* (New York, 1949), pp. 459-471. Of course both Popper and Hempel recognise that explanation and prediction involve differences from the point of view of what is considered as ' known ' and as ' unknown '. Thus Hempel writes (loc. cit.), ' While in the case of an explanation, the final event is known to have happened, and its determining conditions have to be sought, the situation is reversed in the case of a prediction.'

on two counts: (1) it rests upon a tacit but unwarranted assumption as to the nature of the physical universe, and (2) it is inconsistent with scientific custom and usage regarding the concepts of explanation and prediction.[1] In rectification, I shall try to establish that the concept of *evidence* provides the means by which the relationship between explanation and prediction can most effectively be explicated.

Physical systems may be classified into four kinds, according as knowledge of the present state of the system does or does not make possible (1) predictions regarding future states, and/or (2) inferences regarding past states. These four types of systems can be defined, according as the physical laws that characterise the processes of the system are such that, on the basis of complete knowledge of the present state of the system,

Inferences (certain or probable) regarding past states are	*Predictions regarding future states of the system are*
I. Possible	Possible
II. Possible	Not Possible
III. Not Possible	Possible
IV. Not Possible	Not Possible

Certain familiar physical systems—wave propagation and the oscillation of pendula, for example—are defined by equations which are temporarily symmetric, the processes involved being reversible. These systems are of Type I, and their future is as readily determinable as their past. Another commonly considered case is that of open non-equilibrium systems, involving irreversible processes, e.g. goal-directed servo-mechanisms. Here the past of the systems can often be inferred from the present, while their future, in general, cannot, so that these systems are of Type II. Systems of Type III are less familiar, but do certainly exist. In thermodynamics, for example, the prediction of the career of certain *closed* non-equilibrium systems may be possible, whereas post- or retro-diction is not. In view of the little-recognised status of this case, it may be of interest to cite in some detail at least one instance of such a system.

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[1] In a recent article on 'Explanation, Prediction and Abstraction' (this *Journal*, 1957, 7, I). Sheffler adduces a number of considerations which tend to establish this point. (1) Predictions necessarily involve reference to a time of utterance of assertion, while explanations do not. (2) Explanation necessarily involves the giving of reasons, while predictions (e.g. those offered by clairvoyants, prophets or news commentators) need not be reasoned. (3) Only true statements are proper objects for explanation, but clearly not so with prediction. (4) When explanations fail, an error of reasoning is involved, but not so with predictions, and correspondingly, (5) the rational grounds adequate for reasoned justification of a prediction may be insufficient to *explain* the predicted event, should it occur. The analysis of the logic of prediction presented below will serve to throw light upon several of these points.

Consider the diffusion process, with increasing entropy, defined by the following differential equation: [1]

$$\frac{\delta^2 \Psi}{\delta x^2} + \frac{\delta^2 \Psi}{\delta y^2} + \frac{\delta^2 \Psi}{\delta z^2} = a^2 \frac{\delta \Psi}{\delta t}$$

where a^2 is a *real* constant. This diffusion equation differs from the wave equation for a reversible process by having a first time derivative instead of a second. In such cases as, for example, the one-dimensional case of, say, heat flow, the solution of this equation is temporally asymmetric in that : (1) if the system is in equilibrium at time $t = 0$, we cannot infer what particular sequence of non-equilibrium states lead to the present equilibrium condition, no such sequence being unique, and (2) if the system is not in equilibrium at $t = 0$, then it could not have been undergoing diffusion for all *past* values of t, although it can theoretically do so for all future values. Specifically, if external agencies impinge on the system and produce a non-equilibrium state of low entropy at $t = 0$, there is no basis for supposing the system to have been undergoing diffusion before this time, and the diffusion equation cannot be employed to infer the early history of the system on the basis of its state at $t = 0$, although the equation *can* be used to predict its future as a closed system undergoing diffusion. With such a system, then, prediction of the future may be possible, even though retrodiction of the past is not.

Finally, little need (or can) be said about systems of Type IV. Systems which, in the present state of our knowledge, are of this type, represent no more than a category of research problems.

In the final analysis, correct characterisation of the relative status of prediction and explanation hinges upon the proper classification of our physical universe, as characterised by natural science, with respect to these four categories. If, for example, the universe which physics unveils were a Type I system, the status of prediction of future states in terms of the present, and of explanation of past states in terms of preceding ones as inferred from the present, would be wholly analogous. Now this, as we have seen above, has indeed been the position of some philosophers of science. The point which must be stressed, however, is that it is a matter of empirical fact, and not of logical analysis, to establish whether this classification, or some other, is correct. To profess the equivalence of explanation and prediction upon theoretical grounds is to pre-empt an empirical question, by basing the logical analysis upon a hidden assumption as to the character of physical laws, an assumption that properly requires factual (i.e. extra-logical) warrant. There can be no theoretical justification for closing this question of the nature of the physical universe (viewed *in toto* as a closed physical system) by *fiat* in the context of an analysis of explanation and prediction.

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[1] This example is taken from A. Grünbaum's article 'Das Zeitproblem', *Archiv für Philosophie*, 1957, 7, 170-212. A mathematical treatment of the physical considerations is given by F. John, 'Numerical Solution of the Equation of Heat Conduction for Preceding Times', *Annali di Matematica pura ed Applicata*, 1955, 40, 129 sqq. The writer is indebted to Professor Grünbaum for these references, and for informative discussion of the concepts of explanation and prediction, particularly as regards the physical considerations here involved.

It would be well and good to let matters rest here, were not some sort of resolution required for our present task of clarification of the relationship between prediction and explanation. In effecting this necessary resolution, it is best to make, and to make explicit, a working assumption of as modest and as plausible a character as possible. In stating this working assumption, I wish to shift my purview beyond the confines of physical science alone, and to include the biological and social as well as the physical sciences. This shift in the framework of reference is made to obviate the need for any discussion of the thesis of physicalism, i.e. the question whether all departments of natural science are in the last analysis reducible to the physical sciences, in that their concepts and laws are of derivative status. The working assumption is thus intended to apply not to physical and chemical processes alone, but to the whole range of natural science:

Working Assumption

> It is academic to predicate ' total knowledge ' of the present state of a system under consideration, because in the case of natural systems we never *de facto* have such knowledge. However, our knowledge of natural laws is in a great many cases sufficient (albeit with numerous and important exceptions) to underwrite partial, but virtually certain knowledge of the past on the basis of traces found in the present. As regards predictive knowledge, we usually have only fragmentary and in general merely probable knowledge of the future on the basis of knowledge of the present and/or the past, although in certain fields (e.g. astronomy) our knowledge of natural laws does afford virtually certain predictive knowledge.

In view of this working assumption, the foregoing classification of physical systems is effectively rendered inoperative for present purposes, because it involves the assumption of *total knowledge* of the states of systems. The working assumption provides an adequate orientation as to the relation of our (partial) knowledge of the present to that of the future and the past in stipulating the possibility of inferences, based on the use of known natural laws, among non-contemporaneous events. However, an asymmetry in our knowledge of the relation of the present to the future and the part is asserted, and the past is accorded a substantially more favourable position. Let us now consider the bearing of this working assumption on the question of the relationship between explanation and prediction.

An examination of the way in which the term 'explanation' functions both in ordinary, everyday talk and in technical or scientific contexts reveals a great variety of uses. However, one of these usages, and in fact that one which has primary interest and pertinence for the logician and the philosopher of science, is the sense of 'explanation' as a comprehensive and conclusive accounting as to *why* something is the case.[1] There is general agreement that it is an essential characteristic of explanations (in this sense) that they must establish their conclusion (explanans) well-nigh conclusively, i.e. in such a manner that there is little, if any, room left open for reasonable doubt regarding its factuality.[2] Now this requirement, it is clear, is far too stringent to be imposed legitimately upon predictions. If two sides of an otherwise normal die are marked 6, one is justified in predicting 6 as the outcome of a toss of that die. And even our best data for predicting the weather four days hence are almost certain to be insufficient as a basis for explaining the actual weather conditions on that day *ex post facto*.

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[1] It should be noted that this is a *technical* use of 'explanation' and not the ordinary, informal many-purpose version of this term. Thus ordinarily very partial and fragmentary accounts can pass for 'explanations'. One asks 'You were not here on time; explain your lateness!' and is satisfied with the response 'The bus broke down'. Such an 'explanation' does not of course even begin to rule out alternatives that appear reasonable *prima facie*, such as walking in the example.

[2] In their valuable paper on 'Studies in the Logic of Explanation' (cited above) Hempel and Oppenheim suggest that the relationship between explanans and explanandum is that of *logical implication*, in other words, that the explanandum is *deducible* from the explanans. However, I have here taken deliberate care to say that the explanandum must be established 'well-nigh conclusively' on the basis of the explanans, in order to make room for statistical and *inductive* (as well as deductive) explanations.

The thesis I wish to stress is that the reasoned validation of a prediction—the presentation of reasoned justifying arguments in support of the prediction—need do no more than render its conclusion significantly *more likely than its principal alternatives.*[1] In this there resides a crucial difference between predictions and explanations. An adequate explanation must render its conclusion virtually certain, and thus tenable *per se*, while a soundly reasoned prediction need do no more than render its conclusion relatively tenable, i.e. more tenable than alternatives, and to do this in such a way that a *sufficient* (rather than *conclusive*) reason is forthcoming for espousal of the predicted eventuality in preference to the other possibilities. To be sure, many predictions, for example in astronomy, are such as in effect to render their conclusions virtually certain, and such predictions are, so far as concerns the logical structure of their supporting arguments, closely akin to explanations. The point, however, is that not all predictions are of this kind, and that it is not reasonable to require that they should be. This consideration removes the warrant for any wholesale subsumption of prediction under the explanatory rubric.

It is, I believe, plausible to contend that this epistemological difference between predictions and explanations is in no small measure due to the temporal asymmetry inherent—in view of the working assumption—in the fact that the explanation of events is oriented (in the main) towards the past, while prediction is oriented towards the future. Rather than being the single point of minor difference between explanation and prediction, this temporal asymmetry is of far-reaching and fundamental import. Precisely because the past does, as a matter of fact, enjoy a marked superiority over the future as regards the extent of the accessible body of reliable information, doing away with many elements of ignorance, uncertainty, and contingence, is it valid to require explanations to be virtually conclusive. Predictions, by contrast, generally involve us in considering a host of possible alternatives which—even with optimum use of the information at our disposal, in

[1] The adjective 'principal' serves an important rôle here. In considering the outcome of a toss of an otherwise normal die with two sides marked 6 (the 6-side and, say, the usual 5-side), clearly 1-or-2-or-3-or-4 is a better and safer prediction than 6. But the disjunctive description, while indeed an alternative outcome to 6, does not qualify as what I shall call a *principal* alternative of 6, for it is not a strictly comparable outcome. The character of the principal alternatives is to be determined by the intent of the question to which the prediction-statement is a proposed response, and is thus a pragmatic matter in the sense of Morris. It is of the nature of oracular predictions perversely to avoid discrimination among principal alternatives.

fact or even in principle—cannot reasonably be expected to be condensable to some single outcome. In such circumstances we are willing gratefully to accept a reasoned utilisation of the available evidence that can point to some one of the possible cases as meriting tentative acceptance, albeit perhaps in a purely relative manner.[1]

This epistemological asymmetry between explanation and prediction has not been adequately recognised and taken into account in discussions of scientific method. Such recognition would open the way to explicit consideration of a specific methodology of prediction, a matter which seems to date to have been wholly neglected by methodologists and students of scientific method. As long as one believes that explanation and prediction are strict methodological counterparts, it is reasonable to press further solely with the explanatory problems of a discipline, in the expectation that only the tools thus forged will then be usable for predictive purposes. But once this belief is rejected, the problem of a specifically predictive methodology arises, and it becomes pertinent to investigate the possibilities of predictive procedures autonomous of those used for explanation.[2]

It appears that the epistemological concept in terms of which the relationship between explanation and prediction can most effectively be explicated is the concept of supporting evidence. Before entering upon this theme, however, it is necessary briefly to consider some points relating to the logic of evidence.

The epistemological character of the evidential relationship—i.e. the relation obtaining between evidence statements upon the one hand, and a statement supported by them upon the other—is a difficult and ramified subject. The evidence concept covers a wide variety of distinguishable species, including all of the special relationships that obtain when a body of discourse supports some proposition in any of numerous appropriate senses, ranging from the most demanding species of evidence which calls for establishment of a conclusion ' beyond the shadow of a doubt ', to the most provisional and tentative modes of argument, such as analogy or ' circumstantial ' evidence. The evidential relation holds whenever we must give some weight or credence to the conclusion upon some given statements (the evidence) as hypothesis.

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[1] Actually it might be well, as Professor Michael Scriven has proposed to the writer in correspondence, to distinguish explicitly between predictions of two kinds: (1) predictions which select, *faute de mieux*, that principal alternative that is relatively most likely, but whose non-realisation is nonetheless more likely than is its realisation, and (2) predictions whose realisation is claimed to be likely *per se*. However, prediction is a logically weaker procedure than explanation, and this is so in both senses, even in the second, stronger one.

[2] The ideas of this paragraph are the basis of a forthcoming collaborative study in which Dr Olaf Helmer and the writer will analyse the concept of a predictive methodology, and will consider the inherent opportunities for methodological innovations, particularly in the so-called ' inexact ' sciences.

The logical nature of the concept of evidence is a matter greatly in need of analysis and clarification. A study of this relationship, and an endeavour to supplant the purely qualitative concept of ' constituting supporting evidence ' by a qualitative measure of degree of evidential support, is one of the principal tasks of modern formal inductive logic. In recent years important progress towards a solution of this problem has been made, particularly by Popper, Hempel, and Carnap, and the work has also been carried forward by Kemeny and Oppenheim, and others.[1] However, I do not here wish to enter upon this large and technical subject. For my present purposes it will suffice to note in a general way some few characteristics of the concept of supporting evidence that emerge from the logical analysis of this concept.

There are, to be sure, instances of conclusive evidence, evidence which wholly establishes its conclusion. But by adducing evidence in support of some proposition we do not, at least in general, *establish* this proposition. It is only necessary for evidence to render its conclusion more tenable or more likely than before, i.e. more probably *a posteriori* than *a priori*. Evidence, then, is by nature a logically weaker mode of reasoning than proof, in any of the senses of that term. The central and fundamental fact of the theory of evidence is that one statement may constitute evidence for another which goes significantly beyond it in assertive content. This at once differentiates evidence from *entailment*, and differentiates it from *explanation* as well. A true statement may legitimately provide evidence for a falsehood, and one statement may constitute evidence for each of several incompatible statements.[2]

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[1] The pioneering works are Popper's *Logik der Forschung* (Vienna, 1935); Hempel's ' Studies in the Logic of Confirmation ', *Mind*, 1945, **54**, 1-26 and 97-121; and Carnap's *Logical Foundations of Probability* (Chicago, 1950). Other contributions include: Kemeny and Oppenheim, ' Degree of Factual Support ', *Philosophy of Science*, 1952, **19**, 307-342; Popper, ' Degree of Confirmation ', this *Journal*, 1952, **5**, 143-149; a review of the preceding by Kemeny in the *Journal of Symbolic Logic*, 1955, **20**, 304-305; Popper, ' A Second Note on Degree of Confirmation ', this *Journal*, 1957, **7**, 350-353; and the writer's paper, ' A Theory of Evidence ', *Philosophy of Science*, 1958, **25**, January number.

[2] A detailed treatment of these, and analogous considerations can be found in the writer's paper, ' A Theory of Evidence ', cited in the preceding footnote.

The point which deserves emphasis is that evidence is, as it were, a logically weaker counterpart of explanation; it, too, is a type of justifying argumentation, but the logical relationship it establishes is far more tenuous. (For this very reason, its range of application is far wider.) In explaining we must bring forth considerations which render the acceptance of the proposition in view well nigh mandatory. In adducing evidence we merely bring forth considerations which render acceptance of the proposition more palatable than before. Evidence, then, is a mode of reasoning which is a logically weaker cognate of explanation: it is structurally similar in also giving reasons in support of an empirical conclusion, but it may do this so as to furnish its support in a manner far more inconclusive and tenuous than is tolerable for explanation.

Now it is just this distinguishing feature between evidence and explanation which renders the concept of evidence peculiarly fitting for the logical characterisation of reasoned predictions. For we have a reasoned justification of a prediction in precisely those cases in which we are confronted with the evidence relevant to the prediction, and find that this supports the predicted possibility more than its principal alternatives, i.e. when the weight of evidence in favour of the predicted eventuality exceeds the weight of the evidence in favour of the competing candidates (its principal alternatives).

If this description of the rôle of evidence in prediction is correct, it throws light on the points of difference between explanation and prediction that have been discussed above. These differences, it will be recalled, revolved about one primary consideration, namely that while an explanation must render its conclusion virtually certain, it suffices for a reasoned prediction to do no more than render its conclusion more tenable than its alternatives. But once we recognise prediction as a form of evidential reasoning, we immediately account for this point of difference as instancing the characteristic difference between evidence and explanation; yet furthermore, we have available to us at once a plausible account of the similarities between predictions and explanations.

In summary, I wish to recapitulate the principal theses submitted in the present paper:

I. It cannot be maintained that explanation and prediction are identical from the standpoint of their logical structure, the sole point of difference between them being one of content, in that the hypothesis of a prediction concerns the future, while explanations concern the past.

II. In the final analysis, the relationship between explanation and prediction hinges on the proper classification of the physical universe with respect to the capacity of its laws to make possible pre- and retro-diction on the basis of complete knowledge of the ' present ' state of the system.

III. The actual state of our scientific knowledge of the present day being such that an asymmetry in our knowledge of the relation of the present to the future and to the past must be conceded; explanation of the past enjoys a substantially more favourable epistemological position than prediction of the future.

IV. The logical or epistemological requirements imposed upon the concept of prediction by its accepted usage in scientific and technical contexts is such as to bear out the thesis that the justification of predictions is a logically weaker and less conclusive mode of reasoning than explanation.

V. The epistemological concept in terms of which the relationship between explanation and prediction can most effectively be explicated is the concept of *supporting evidence*. Indeed, it is necessary in the interests of logical taxonomy to reclassify prediction as an evidential, rather than as an explanatory mode of reasoning.

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NOTES AND COMMENTS

Instrumentalism and Relativity

1 *Introduction*

K. Popper,[1] has recently invented the term ' instrumentalism ' to describe *that philosophy which regards physical science merely as a convenient instrument for making calculations to predict observable phenomena.* According to instrumentalism the task of the theoretical physicist is to create the relevant mathematical formalism and to apply it to the data at hand ; physical theories cannot in any sense be said to form a true description of the world : they are simply devices for calculating, and rules of inference.

It is characteristic of instrumentalism that a new theory is not regarded as falsifying an old one, but rather as having a different range of applicability. For example, the theory of relativity is not thought of as proving Newtonian mechanics untrue : it merely replaces the older theory for calculations about certain phenomena. According to instrumentalism neither theory is true or false, but both are convenient for certain applications. It is to this attitude which Popper objects most strongly, arguing that instrumentalism neglects falsification and is therefore ' obscurantist ' ; and that it may lead to the stagnation of physics.

Whilst I think that Popper's criticism has a certain weight, I do not find it completely convincing for the following reason. Confronted with a discrepancy between a theory and an observation, an instrumentalist, like anybody else, will take heed of it, and will demand a new theory. In this sense, therefore, instrumentalism does not neglect falsification. What distinguishes the instrumentalist is that he will not wish to bother with the question whether the new theory is ' true ' and the old ' false ', but will study instead the range of applicability of the theories. Now this latter pursuit, though less glamorous than the search for ' absolute truth ', may still be very compelling, and may lead the instrumentalist to extremely delicate and subtle investigations. For this reason it does not seem at all necessary that instrumentalism should lead to the stagnation of physics.

In this paper I wish to put forward another criticism of instrumentalism. My argument will be that it does not give a correct account of the professional activities of a considerable class of physicists, and for this reason it is not a satisfactory basis for a philosophy of physics.

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[1] K. Popper, *Contemporary British Philosophy*, Muirhead Library of Philosophy, 1956, p. 357

As a definition of instrumentalism I shall take the italicised part of the first sentence of this article.

2 *Function of a Philosophy of Science*

It would, I think, be generally agreed that it is not the function of a philosopher of science to tell scientists what they should or should not do in their professional work. The philosopher's task is rather that of an observer watching the scientist at work, noting the characteristics of the scientific method, and bringing to light the underlying logical assumptions. He is, so to speak, an anthropologist studying the scientific community in action, without passing judgments on scientific mores, bizarre though they may sometimes seem. This is not to say, of course, that the philosopher may not occasionally point out inconsistencies, and confusions of language and understanding, which appear in scientific work. In general, however, he will be content with his anthropological rôle, and will control any desire he may have to become instead a missionary.

If this assessment is right, it follows that an essential requirement of a philosophy of physics is that it gives a correct description of the aims and methods of the science as it is actually practised. My ground for criticising instrumentalism is that it does not do this, as I hope to show in the next section.

3 *Instrumentalism and Relativity*

I wish to discuss instrumentalism in connection with scientific method as practised by workers in the theory of gravitation during the past forty years. The empirical demands on a gravitation theory at the present time are the following. In the weak-field approximation it must reduce to Newton's theory, and in the absence of gravitation it must give the special theory of relativity. Further, it must predict the three 'crucial tests' concerning the astronomy of the solar system,[1] and give a cosmological model which accounts for the red-shift of the distant nebulae, and for certain other cosmological data. General relativity was the first theory to satisfy these demands, but there are two other theories, due respectively to A. N. Whitehead [2] and to G. D. Birkhoff [3], which also satisfy them. Of these three theories, general relativity, which uses non-linear differential equations, is by far the most complicated from a manipulative point of view.

[1] There is some doubt as to how far general relativity accords with observation in two of the three crucial tests. It is unnecessary to discuss this further here: the point essential to my argument is that if general relativity is at variance with observation, so are the alternative theories.

[2] A. N. Whitehead, *The Principle of Relativity*, Cambridge, 1922

[3] G. D. Birkhoff, *Proceedings of the National Academy of Sciences*, 1943, **29**, 231

Yet, and this is my main point, *general relativity is the theory which commands almost universal acceptance, and which would undoubtedly be tried first to predict new observable gravitational phenomena.*[1] Now according to instrumentalism one would expect that the theories of Whitehead and Birkhoff, which are much more convenient instruments for calculation, would be the accepted theories. Thus instrumentalism does not accord with scientific practice in gravitation theory.

It might of course be argued that this dilemma, in which several theories satisfy the empirical evidence, is a temporary one, and that eventually further data will be found to settle the matter. This is no doubt true, but as the situation described has been with us for about thirty years, it seems that the philosophy of science must allow for it. Indeed, if only one theory satisfies the empirical evidence, everyone will agree that this is the proper theory to choose, at least for the time being, and there is no problem. It is precisely when there is a possible choice of theories that the philosophy of science can be useful in helping to decide between them, or at least in clarifying the logical bases of the theories, and in seeing what their adherents commit themselves to. Instrumentalism, when appealed to in this way concerning gravitation theory offers a solution which no serious worker in the subject feels able to accept.

In passing, it is worth noticing that the appeal to philosophy to decide between rival theories, all satisfying the available evidence, is frequently made in cosmology. In this subject the scientists themselves frequently conduct the philosophical arguments, sometimes almost without realising it. An example of the valuable contribution which the philosopher of science can make to the clarification of the issues involved is given in the work of M. K. Munitz.[2]

Why is general relativity, in spite of its complexity, accepted in preference to the other theories? Most workers in the field would probably say, vaguely, that it is because of the greater logical coherence and inner consistency which general relativity seems to have. In so far as its field equations can be made to yield the equations of motion of particles without further postulate, one can say that general relativity has an advantage in the economy of hypothesis. But this is not the whole story by any means. The way in which general relativity deals with the equality of gravitational and inertial mass is widely held to be one of its most cogent features, yet it is not clear that its advantage over the other theories in this respect is simply that of economy. Another different reason for confidence is that, granted

[1] At the present time there is much interest in the possible existence of gravitational waves, a phenomenon with which the theories of Whitehead and Birkhoff are formally well equipped to deal because of their linearity. Yet all the investigators use general relativity in spite of the great complications involved.

[2] M. K. Munitz, *Space, Time and Creation*, Illinois, 1957

the underlying assumptions, one is led almost uniquely and without ambiguity to the field equations of general relativity, whereas Whitehead's theory, for example, is one of an infinite class of similar theories.[1]

It is considerations such as these which more than compensate for the difficulties in calculation involved in general relativity. Such matters are quite outside the scope of instrumentalism and show that it is altogether incapable of giving a proper assessment of the value of a scientific theory such as relativity.

The argument can be looked at in another way. General relativity has proved, so far, a 'convenient instrument' for predicting the three crucial tests and certain cosmological observations. This, according to instrumentalism, is the present extent of its significance. The fact that it has revolutionised our outlook on space and time and on the notion of force, carries no weight whatever. Clearly a philosophy which considers a scientific theory in this naïve way does not begin to help us understand what science is about.

4 *Conclusion*

As a physical theory must be capable of verification by an appeal to observation, instrumentalism is right in requiring a theory to act as a means of predicting observable phenomena. If my argument above is correct it follows that this requirement, though a necessary one for a successful theory, is not sufficient. Evidently there are other features of general relativity which have made physicists believe that it is successful. An account of scientific method should allow for such additional features ; as it fails to do this I conclude that instrumentalism is not a satisfactory philosophy of science.

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A Third Note on Degree of Corroboration or Confirmation

IN this note I wish to make a number of comments on the problem of the *weight of evidence*, and on *statistical tests*.

1 The theory of corroboration or 'confirmation' proposed in my two previous Notes on 'Degree of Confirmation'[2] is able to solve with ease the so-called *problem of the weight of evidence*.

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[1] A. Schild, *Proceedings of the Royal Society, A.* **235,** 1956, 202

[2] This *Journal*, 1954, 5, 143, 324, and 359 ; and 1957, **7,** 350. See also 1955, **6,** and 1956, **7,** 244, 249. To the first paragraph of my 'Second Note', a reference

This problem was first raised by Peirce, and discussed in some detail by Keynes who usually spoke of 'the weight of an argument' or of the 'amount of evidence'. The term 'weight of evidence' is taken from J. M. Keynes, and from I. J. Good.[1] Considerations of the 'weight of evidence' lead, within the subjective theory of probability, to paradoxes which, in my opinion, are insoluble within the framework of this theory.

2 By the subjective theory of probability, or the subjective interpretation of the calculus of probability, I mean a theory that interprets probability as a measure of our ignorance, or of our partial knowledge, or of the degree of the rationality of our beliefs, in the light of the evidence available to us.

I may mention, in parentheses, that the more customary term, 'degree of rational belief', is indicative of a slight confusion since what is intended is 'degree of the rationality of a belief'. The confusion arises as follows. Probability is first explained as a measure of the strength or intensity of a belief or conviction (measurable, say, by our readiness to accept odds in betting). Next it is realised that the intensity of our belief often depends, in fact, upon our wishes or fears rather than upon rational arguments; thus, by a little change, probability is then interpreted as the intensity, or the degree, of a belief *in so far as it is rationally justifiable.* But at this stage, the reference to the intensity of a belief, or to its degree, clearly becomes redundant; and 'degree of belief' should therefore be replaced by 'degree of the rationality of a belief'. (These remarks should not be taken to mean that I am prepared to accept *any* form of the subjective interpretation; see point 12, below, and chapter ii of my *Postscript: After Twenty Years* to *The Logic of Scientific Discovery.*)

3 In order to save space, I shall explain the problem of the weight of evidence merely by way of presenting one of the paradoxes to which I referred above. It may be called the '*paradox of ideal evidence*'.

Let z be a certain penny, and let a be the statement 'the nth (as yet unobserved) toss of z will yield heads'. Within the subjective theory, it may be assumed that the absolute (or prior) probability of the statement a is equal to $\frac{1}{2}$, that is to say,

$$P(a) = \tfrac{1}{2} \qquad (1)$$

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should be added to a paper by R. Carnap and J. Bar-Hillel, 'Semantic Information', this *Journal*, 1953, 4, 147 sqq. Moreover, the first sentence of note 1 on p. 351 should read, '*Op. cit.*, p. 83', rather than as at present, because the reference is to Dr Hamblin's thesis.

[1] Cf. C. S. Peirce, *Collected Papers*, 1932, Vol. 2, p. 421 (first published 1878); J. M. Keynes, *A Treatise on Probability*, 1921, pp. 71 to 78 (see also 312 sq., 'the amount of evidence', and the *Index*); I. J. Good, *Probability and the Weight of Evidence*, 1950, pp. 62 f. See also C. I. Lewis, *An Analysis of Knowledge and Valuation*, 1946, pp. 292 sq.; and R. Carnap, *Logical Foundations of Probability*, 1949, pp. 554 sq.

Now let e be some *statistical evidence*; that is to say, a *statistical report*, based upon the observation of thousands or perhaps millions of tosses of z, and let this evidence e be *ideally favourable* to the hypothesis that z is strictly symmetrical—that it is a 'good' penny, with equidistribution. (Note that e is here not the full, detailed report about the results of each of these tosses—this report we might assume to have been lost—but only a *statistical abstract* from the full report; for example, e may be the statement, 'in a million of observed tosses of z, heads occurred in 500,000 ± 20 cases'. It will be seen, from point 8, below, that an evidence e' with 500,000 ± 1,350 cases would still be ideal, if my functions C and E are adopted; indeed, from the point of view of these functions, e is ideal precisely because it entails e'.) We then have no other option concerning $P(a, e)$ than to assume that

$$P(a, e) = \tfrac{1}{2}. \tag{2}$$

This means that the probability of tossing heads remains unchanged, in the light of the evidence e; for we now have

$$P(a) = P(a, e). \tag{3}$$

But this formula has to be interpreted as asserting that e is, on the whole, (absolutely) *irrelevant* information with respect to a.

Now this is a little startling; for it means, more explicitly, that our so-called '*degree of rational belief' in the hypothesis, a, ought to be completely unaffected by the accumulated evidential knowledge, e*; that the absence of any statistical evidence concerning z justifies precisely the same 'degree of rational belief' as the weighty evidence of millions of observations which, corroborate or *prima facie*, support or confirm or strengthen our belief.

4 I do not think that this paradox can be solved within the framework of the subjective theory, for the following reason.

The *fundamental postulate of the subjective theory* is the postulate that degrees of the rationality of beliefs in the light of evidence exhibit a *linear order*: that they can be measured, like degrees of temperature, on a one-dimensional scale. But from Peirce to Good, all attempts to solve the problem of the weight of evidence within the framework of the subjective theory proceed by introducing, in addition to probability, *another measure of the rationality of belief in the light of evidence*. Whether this new measure is called 'another dimension of probability', or 'degree of reliability in the light of the evidence', or 'weight of evidence' is quite irrelevant. Relevant is merely the implicit admission that it is not possible to attribute linear order to degrees of the rationality of beliefs in the light of the evidence: that there may be *more than one way in which evidence may affect the rationality of a belief*. This admission is sufficient to overthrow the fundamental postulate on which the subjective theory is based.

Thus the naïve belief that there really are intrinsically different kinds of entities, some to be called, perhaps, 'degree of rationality of belief' and others 'degree of reliability' or of 'evidential support', is no more able to rescue the subjective theory than the equally naïve belief that these various measures 'explicate' different 'explicanda'; for the claim that there exists an 'explicandum' here—such as 'degree of rational belief'—capable of 'explication' in terms of probability stands and falls with what I have called the 'fundamental postulate'.

5 All these difficulties disappear as soon as we interpret our probabilities *objectively*. (It does not matter, in the context of the present paper, whether the objective interpretation is a *purely* statistical interpretation or a propensity interpretation.[1]) According to the objective interpretation, we have to introduce b, the statement of the conditions of the experiment (the conditions defining the sequence of the experiments from which we take our example); for instance, b may be the information: 'the toss in question will be a toss of z, randomised by spinning'. Moreover, we have to introduce the *objective* probabilistic hypothesis h, that is to say, the hypothesis, '$P(a, b) = \frac{1}{2}$'.[2]

From the point of view of the objective theory, what we are mainly interested in is the hypothesis h, that is to say, the statement

$$`P(a, b) = \tfrac{1}{2}'.$$

6 If we now consider the ideally favourable statistical evidence e which led to the 'paradox of ideal evidence', it is quite obvious that from the point of view of the objective theory, e is to be considered as evidence bearing upon h rather than evidence bearing upon a: it is ideally favourable to h, and quite neutral to a. Upon the assumption that the various tosses are *independent, or random*, the objective theory yields for *any* statistical evidence e quite naturally $P(a, be) = P(a, b)$; thus e is indeed irrelevant to a, in the presence of b.

Since e is evidence in favour of h, our problem turns, as a matter of course, into one of asking how the evidence e corroborates h (or 'confirms' h). The answer is that if e is ideally favourable evidence, then both $E(h, e)$ and $C(h, e)$, i.e., the degree of corroboration of h, given e, will approach 1, if the size n of the sample upon which e is based goes to infinity.[1] Thus ideal evidence produces a correspondingly ideal behaviour of E and C. Consequently, no paradox arises; and we may quite naturally measure *the weight of the evidence e with respect to the hypothesis h* by either $E(h, e)$, or $C(h, e)$ or else—keeping more closely to Keynes' idea—by the absolute values of either of these functions.

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[1] For the 'propensity interpretation' of probability, see my papers 'Three Views Concerning Human Knowledge'; 'Philosophy of Science: A Personal Report'; and 'The Propensity Interpretation of Probability and the Quantum Theory', published, respectively, in *Contemporary British Philosophy*, ed. by H. D. Lewis; in *British Philosophy in the Mid-Century*, ed. by C. A. Mace; and in *Proceedings of the Ninth Symposium of the Colston Research Society*, 1957 (*The Colston Papers*, 9), ed. by S. Körner.

[2] Note that 'b' may be interpreted, alternatively, not as a name of a statement but as a name of the sequence of tosses—in which case we would have to interpret 'a' as a name of a class of events rather than as a name of a statement; but 'h' remains the name of a statement in any case.

7 If, as in our case, h is a statistical hypothesis, and e the report of the results of statistical tests of h, then $C(h, e)$ is a measure of the degree to which these tests corroborate h, exactly as in the case of a non-statistical hypothesis.

It should be mentioned, however, that as opposed to the case of a non-statistical hypothesis, it might be at times quite easy to estimate the numerical values of $E(h, e)$ and even of $C(h, e)$, if h is a statistical hypothesis.[2] (In 8 I will briefly indicate how such numerical calculations might proceed in simple cases, including, of course, the case of $h =$ '$P(a, b) = 1$'.)

The expression

$$P(e, h) - P(e) \tag{4}$$

is decisive for the functions $E(h, e)$ and $C(h, e)$; indeed, these functions are nothing but two different ways of 'normalising' the expression (4); they thus increase and decrease with (4). This means that in order to find a *good* test-statement e—one which, if true, is highly favourable to h—we must construct a statistical report e such that (i) e makes $P(e, h)$—which is Fisher's 'likelihood' of h given e—large, i.e. nearly equal to 1, and such that (ii) e makes $P(e)$ small, i.e., nearly equal to 0. Having constructed a test statement e of this kind, we must submit e itself to empirical tests (That is to say, we must *try* to find evidence refuting e.)

Now let h be the statement

$$P(a, b) = r \tag{5}$$

and let e be the statement 'In a sample which has the size n and which satisfies the condition b (or which is taken at random from the population

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[1] Both E and C are defined in my first note. It is sufficient here to remember that $E(h\ e) = (P(e, h) - (P(e))/(P(e, h) + P(e))$, and that C approaches E in most cases of importance. In this *Journal*, 1954, 5, 324, I suggested that we define

$$C(x, y, z) = (P(y, xz) - P(y, z))/(P(y, xz) - P(xy, z) + P(y, z)).$$

From this we obtain $C(x, y)$ by assuming z (the background knowledge) to be tautological.

[2] It is quite likely that in numerically calculable cases, the logarithmic functions suggested by Hamblin and Good (see my 'Second Note') will turn out to be improvements upon the functions which I originally suggested. Moreover, it should be noted that from a numerical point of view (but not from the theoretical point of view underlying our *desiderata*) my functions and the 'degree of factual support' of Kemeny and Oppenheim will in most cases lead to similar results.

b), *a* is satisfied in $s(r \pm \delta)$ of the instances'. Then we may put, especially for small values of δ,

$$P(e) \approx 2\delta. \qquad (6)$$

We may even put $P(e) = 2\delta$; for this would mean that we assign equal probabilities—and therefore, the probabilities $1/n$—to each of the possible proportions, $1/n, 2/n, \ldots n/n$, with which a property *a* may occur in a sample of the size *n*. It follows that we would have to assign the probability, $P(e) = (2d + 1)/n$, to a statistical report *e* informing us that $m \pm d$ members of a population of the size *n* have the property *a*; so that, by putting $\delta = (d + \frac{1}{2})/n$, we obtain $P(e) = 2\delta$. (The equidistribution here described is the one which Laplace assumes in the derivation of his rule of succession. It is adequate for assessing the absolute probability, $P(e)$, if *e* is a *statistical report about a sample*. But it is inadequate for assessing the relative probability $P(e, h)$ of the same report, given a hypothesis *h* according to which the sample is the product of an *n* times repeated experiment whose possible results occur each with a certain probability. For in this case, it is adequate to assume a combinatoric, i.e. a Bernoullian, rather than a Laplacean, distribution.) We see from (6) that, if we wish to make $P(e)$ small, we have to make δ small.

On the other hand, $P(e, h)$—the likelihood of *h*—will be close to 1 *either* if δ is comparatively large (roughly, if $\delta \approx \frac{1}{2}$) *or*—in case δ is small —if *n*, the sample size, is a large number. We therefore find that $P(e, h) - P(e)$, and thus our functions *E* and *C*, can only be large if δ is small and *n* large; or in other words, if *e* is a statistical report asserting a good fit in a large sample.

Thus the test-statement *e* will be the better the greater its precision (which will be inverse to 2δ) and consequently its refutability or content, and the larger the sample size *n*, the statistical material required for testing *e*. And the test-statement *e* so constructed may then be confronted with the results of actual observations.

We see that accumulating statistical evidence will, if favourable, increase *E* and *C*. Accordingly, *E* or *C* may be taken as measures of the weight of the evidence in favour of *h*; or else, their absolute values may be taken as measuring the weight of the evidence with respect to *h*.

8 Since the numerical value of $P(e, h)$ can be determined with the help of the binomial theorem (or of Laplace's integral), and since especially for a small δ we can, by (6), put $P(e)$ equal to 2δ, it is possible to calculate $P(e, h) - P(e)$ numerically, and also *E*.

Moreover, we can calculate for any given *n* a value $\delta = P(e)/2$ for which $P(e, h) - P(e)$ would become a maximum. (For $n = 1{,}000{,}000$, we obtain $\delta = 0{\cdot}0018$.) Similarly, we can calculate another value, of $\delta = P(e)/2$, for which *E* would become a maximum. (For the same *n*, we obtain $\delta = 0{\cdot}00135$, and $E(h, e) = 0{\cdot}9946$.)

For a universal law h such that $h =$ '$P(a, b) = 1$' which has passed n severe tests, *all* of them with the result a, we obtain first, $C(h, e) = E(h, e)$, in view of $P(h) = 0$; and further, evaluating $P(e)$ with the help of the Laplacean distribution, and $d = 0$, we obtain $C(h, e) = (n - 1)/(n + 1) = 1 - 2/(n + 1)$. It should be remembered, however, that non-statistical scientific theories have, as a rule, a form totally different from the h here described, and that, if they are put into this form, any instance a, and therefore e, would become essentially non-observational.

9 One may see from all this that the testing of a statistical hypothesis is deductive—as is that of all other hypotheses: first a test-statement is constructed so that it follows (or almost follows) from the hypothesis, although its content or testability is high; and afterwards it is confronted with experience.

It is interesting to note that if e were chosen so as to be a full report of our observations—say, a full report about a long sequence of tosses, head, head, tail, . . ., etc., a sequence of one thousand elements—then e would be useless as evidence for a statistical hypothesis; for *any* actual sequence of the length n is equally probable to any other sequence (given h). Thus we should arrive at the same value for $P(e, h)$, and thus for E and C, whether the sequence e contains, say, *only* heads, or whether it contains exactly half heads and half tails. This shows that we cannot make use, as evidence for or against h, of our *total* observational knowledge, but, that we must extract, from our observational knowledge, such *statistical* statements as can be compared with statements which either follow from h, or which have at least a high probability, given h. Thus if e consists of the complete results of a long sequence of tosses, then e is, *in this form*, completely useless as a test-statement of a statistical hypothesis. But a logically weaker statement of the *average frequency* of heads, extracted from the same e, could be used. For a probabilistic hypothesis can explain only *statistically interpreted* findings, and it can therefore be tested and corroborated only by statistical abstracts—and not, for example, by the 'total available evidence', if this consists of a full observation report; not even if its statistical interpretations may be used as excellent and weighty test-statements.

Thus our analysis shows that statistical methods are essentially hypothetico-deductive, and that they proceed by the elimination of inadequate hypotheses—as do all other methods of science.

10 If δ is very small, and therefore also $P(e)$, we have, in view of (6),

$$P(e, h) \approx P(e, h) - P(e). \tag{7}$$

In this case, and only in this case, it will therefore be possible to accept Fisher's likelihood function as an adequate measure of degree of corroboration. We can interpret, *vice versa*, our measure of degree of corroboration as a generalisation of Fisher's likelihood function; a generalisation which covers cases, such as a comparatively large δ, in which Fisher's likelihood function would become clearly inadequate. For the likelihood of h in the light of the statistical evidence e should certainly not reach a value close to its maximum merely because the available statistical evidence e was lacking in precision.

It is unsatisfactory, not to say paradoxical, that statistical evidence e, based upon a million tosses and $\delta = 0{\cdot}00135$, would result in numerically *the same likelihood*—i.e., 0·9930—as would statistical evidence e', based on only a hundred tosses and $\delta = 0{\cdot}135$. (But it is quite satisfactory to find that $E(h, e) = 0{\cdot}9946$ while $E(h, e') = 0{\cdot}7606$.)

11 It should be noticed that the absolute logical probability of a universal law h—that is, $P(h)$—will be in general zero, in an infinite universe. For this reason, $P(e, h)$—that is, the likelihood of h—will become indefinite, in most systems of probability, since in most systems $P(e, h)$ is defined as $P(eh)/P(h) = 0/0$. We therefore need a formal calculus of probability which yields definite values for $P(e, h)$ even if $P(h) = 0$, and which will always and unambiguously yield $P(e, h) = 1$ whenever e follows (or 'almost follows') from h. A system answering these demands was published by me some time ago.[1]

12 Our $E(h, e)$ may be adequately interpreted as a measure of the explanatory power of h with respect to e, even if e is not a report of genuine and sincere attempts to refute h. But our $C(h, e)$ can be adequately interpreted as degree of corroboration of h—or of the rationality of our belief in h in the light of tests—only if e consists of reports of the outcome of sincere attempts to refute h, rather than of attempts to verify h.

As hinted in the preceding sentence, I suggest that, while it is a mistake to think that probability may be interpreted as a measure of the rationality of our beliefs (this interpretation is excluded by the paradox of perfect evidence), degree of corroboration may be so interpreted.[2] As to the calculus of probability, it can be interpreted by a large number of different interpretations.[3] Although 'degree of rational belief' is not among them, there is a *logical interpretation* which takes probability as a generalisation of deducibility. But this probability logic has little to do with our hypothetical estimates of chances or of odds; for the probability statements in which we express these estimates are always hypothetical appraisals of the *objective possibilities* inherent in the particular situation—in the objective conditions of the situation, for example in the experimental set-up. These hypothetical estimates (which are *not derivable* from anything else, but freely proposed, although they may be suggested by symmetry considerations, or by statistical material) can in many important cases be submitted to statistical tests. They are never estimates of our own nescience: the opposite view, as Poincaré saw so clearly, is the consequence of a possibly unconscious determinist view of the world.[1]

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[1] This *Journal*, 1955, **6**; see esp. 56 sq. A simplified form of this axiom system may be found in my papers 'Philosophy of Science: A Personal Report' (p. 191) and 'The Propensity Interpretation', etc., referred to in note 3 above. (In the latter paper, p. 67, note 3, the last occurrence of '$<$' should be replaced by '$\neq$', and in (B) and (C) a new line should commence after the second arrows.)

[2] Cf. this *Journal*, 1955, 55 (the title of section 3).

[3] Cf. my note in *Mind*, 1938, **47**, 275 sq.

From this point of view, a ' rational better ' always tries to estimate the *objective odds*. The odds which he is ready to accept do not represent a measure of his ' degree of belief ' (as is usually assumed), but they are, rather, the object of his belief. He believes that there are, objectively, such odds : he believes in a probability hypothesis h. If we wish to measure, behaviouristically, the degree of his belief (in these odds or in anything else) then we would have to find out, perhaps, what proportion of his fortune he is ready to risk on a one-to-one bet that his belief—his estimate of the odds—was correct, provided that this can be ascertained.

As to degree of corroboration it is nothing but a measure of the degree to which a hypothesis h has been tested, and of the degree to which it has stood up to tests. It must not be interpreted, therefore, as a degree of the rationality of our belief in the *truth* of h ; indeed, we know that $C(h, e) = 0$ whenever h is logically true. Rather, it is a measure of the rationality of *accepting*, tentatively, a problematic guess, knowing that it is a guess, but one which has been submitted to searching examination.

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On the ' Space ' and ' Time ' of Hallucinations *

SMYTHIES, whose interesting paper on the mescaline hallucinations appeared in this *Journal*,[2] has recently argued that ' Hallucinations are spatial and coloured entities and may possess not only a high degree of internal organisation, but may also be closely integrated into the " veridical " remainder of the visual field in which they occur '.[1] He conjectures that ' perceptual and hallucinatory spaces ' may not be coincident with 3-dimensional physical space but may be component sub-spaces of a *n*-dimensional manifold which includes physical space as well. All ' causal ' transactions between ' perceptual spaces ' and physical space may involve ' interfaces ' in the multidimensional *n*-space. I should like to raise some geometrical and epistemological questions about the alleged ' space ' and ' time ' of hallucinations.

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[1] Cf. H. Poincaré, *Science and Method*, 1914, IV, I. (This chapter was first published in *La Revue du mois*, 1907, 3, 257-276, and in *The Monist*, 1912, **22**, 31-52.)

* I am grateful to Dr Adolf Grünbaum for sending me a pre-publication copy of his paper with the title ' Carnap's Views on the Foundations of Geometry ', appearing in *The Philosophy of Rudolf Carnap*, ed. P. A. Schilpp, ' Library of Living Philosophers ', Volume 10. I thank Dr Gardner Murphy, Director of Research, Menninger Foundation, Topeka, Kansas, U.S.A., for the encouragement I received from him in my studies.

[2] J. R. Smythies, ' The Mescaline Phenomena ', this *Journal*, 1953, 3, 338-347

' Dimension ', in modern mathematics, is basically a topological concept. The usual applications of it involve issues about the ' metrisability ' of spaces. All topologically equivalent structures must be equi-dimensional, but the converse is not necessarily true. Even if ' hallucinatory ' and ' ordinary perceptual ' spaces are equi-dimensional, their topological equivalence is far from demonstrated. There is no meaning, so far as I am able to judge, in arbitrarily producing a class of objects (' hallucinatory percepts ', ' imagy constituents of mind ', etc.) and asking us whether the class can be given the form of a metric without specifying further conditions. We can talk only about the ' metrisability ' of a space with a *given* topology which should be Hausdorff and completely normal. There exist general topological spaces for which we may adopt Menger-Brouwer definitions of ' dimension ' ; but it would not be at all easy to deduce for all of them *theorems* about ' dimensionality '. Hurewicz and Wallman have reminded us that even for so ' regular ' a space as a bicompact (' bicompact ' in the Alexandroff-Urysohn sense is ' compact ' in Bourbaki's sense) Hausdorff space, it may happen that a *subset* has a *greater* number of dimensions than the set. Smythies speculates on the ' intersection ' of ' subsets ' in his ' *n*-dimensional space ' without stopping to notice the extremely complicated topological questions that the hypothesis would force on us.

The ' space of ordinary perception ' is endowed with a metric largely on the basis of measurements conducted in the light of various conventions and postulates. The general statement requires no partisan advocacy of a particular epistemological theory of space perception. Grünbaum has recently maintained that once a specified co-ordinating definition of geometric congruence has been prescribed (however arbitrarily, according to extreme conventionalists, positivists, and operationists), the coincidence relations among the points described by experience and measurement yield a metric for the space. Only *after* a semantical interpretation of the postulates and theorems relating to a geometry has been provided, can experience be said to ' verify ' or ' falsify ' the hypothesis.

On the basis of *what* measurements and conventions, I ask, can the ' metrisability ' and ' dimensionality ' of the ' space ' of ' mescal visions ' be determined ? Under normal conditions, perceptible size first remains constant and then decreases as distance is increased and the effects of convergence and accommodation and of binocular disparity (their precise rôle is still disputed) become less appreciable. Piéron[1] remarks that, under some artificial conditions, the size of the image can be made to vary inversely with apparent distance. For after-images, as data furnished by Schmülling and Klüver showed, size generally increases with distance. Carr found that distance increases only at a slightly faster rate than size for the first thirty feet. Emmert's law, which states that the relation between the size of the after-image and distance remains constant as distance is varied, has been elucidated by Boring[2] as a special case of ' size constancy ' when certain commonly accepted assumptions (e.g. about the linear size of the stimulus-object and distance from the nodal point) are made. The ' constancies ' of ' ordinary perception ' are significant not only for overt behaviour but for the *geometrical interpretation* of the perceived world.

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[1] J. R. Smythies, *Analysis of Perception*, London, 1956, Chapter IV, p. 81

Experts on visual perception like Vernon[3] have doubted whether even the stable, accurate, and complementarily-coloured eidetic images, which approximate to after-images, conform to Emmert's law. Koffka[4] referred to observations implying that the size of the after-image perceived against a plane surface, on which a perspective drawing of a tunnel had been made, varied with the perspective point with which the image was associated. I should judge that the situation in respect of apparent size, position, distance, is desperately vague when we turn to intricate mescal hallucinations. Klüver[5] admitted great ' difficulties of observation ' ; the form and colour of after-images transformed themselves into the ' visionary phenomena ' ; ' surface colours ' tended to become ' film colours '; the determination of apparent ' size ' was ' very difficult under the conditions ' ; the approximate ' constancy of size ' found in some eidetic experiments did not seem to hold. In a later report Klüver[6] remarked that although certain ' form constants ' (like ' lattice ', ' cobweb ', ' tunnel ', ' spiral ') were comparatively frequent in mescaline hallucinations, there were perplexing alterations in the number, size, and shape of objects (polyopia, dysmegalopsia, dysmorphopsia). All objects may appear to be situated at the same indefinite distance ; or, they may recede into space without altering their size (porrhopsia). Fernberger[1] reported a 'distortion of space' for both monocular and binocular vision. The term 'mescaline hallucinations' can, in fact, be used only in a perfunctory way to characterise the phenomena. The self-observations of even competent investigators have by no means settled the dispute whether phenomena arising at certain stages should be called 'hallucinations' or 'pseudo-hallucinations' (in Kandinsky's or some other sense). My point is that there are no criteria (comparable to those available for 'ordinary perception') by which we can judge the *geometry* of 'hallucinatory spaces'. The 'conscious continuity' of 'veridical' and 'hallucinatory' perception, to which Smythies appeals, does not establish *geometric continuity* in some unambiguous sense.

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[1] H. Piéron, *The Sensations : their Functions, Processes and Mechanisms*, revised edition, New Haven, 1952, p. 223, footnote 1

[2] E. G. Boring, ' Size Constancy and Emmert's Law ', *Amer. J. Psychol.*, 1940, **53**, 293-295

[3] M. D. Vernon, *Visual Perception*, Cambridge, 1937, p. 199

[4] K. Koffka, *Principles of Gestalt Psychology*, New York, 1935, p. 212

[5] H. Klüver, ' Mescal Visions and Eidetic Vision ', *Amer. J. Psychol.*, 1926, **37**, 502-515

[6] H. Klüver, ' Mechanisms of Hallucinations ' in *Studies in Personality* (in honour of Lewis M. Terman), ed. Q. McNemar and M. A. Merrill, New York, 1942, Chapter X

Smythies neglects too the technical issues about the 'time-ordering' of 'hallucinatory worlds'. It is with the aid of conventional time-measurers or 'clocks' (however arbitrarily graduated initially, according to Milne) that we are able to introduce a metric at all into our awareness of 'passage' and describe events as forming a 'well-ordered', one-dimensional sequence. The extremely unstable character of the 'time' of mescaline hallucinations, of which observers like Favilli, Heffter, Fernberger and Klüver have spoken, reinforces the lesson about the instability of their 'space'. It is far from clear whether Smythies regards 'time' as one or many of the 'dimensions of his '*n*-space'.[2] If we *assume* that the class of subsets T of a Hausdorff space Σ is linearly ordered (i.e. $T_1 < T_2$), it follows that it has a topology, the topology of linear order. A necessary and sufficient condition for pronouncing that this topology is not inducible by a metric is that the class T is not separable ; i.e. no subset of the class is dense in the class. Of course, if the original Hausdorff space is endowed with a metric, then the family of its dense subsets T can also be given the form of a metric. But when we are dealing with the 'space' and 'time' of 'mescaline hallucinations', what are the compelling reasons for *assuming* a metric at the start ? How, on the basis of the empirical data adduced by Smythies, can we rule out the possibility that some 'hallucinatory spaces' are *not* 'length-preserving' and 'angle-preserving' ? In affine geometry only *parallel* line-segments can be measured against one another.

It seems to me that, in any case, Smythies may have to face serious, perhaps insuperable, difficulties about 'causality' operating in his '*n*-space'. The late Hermann Weyl,[3] basing himself on Hadamard's argument that Huyghens' principle for the propagation of a spherical light-wave is satisfied only by spaces of an *odd* number of dimensions, suggested that there may be an intimate connection between 'causality' and the contingent number of space-time 'dimensions' in our experience. On the 'representative theory' sketched by Smythies, we have three dimensions of physical space and three more of perceptual space (confining ourselves to one percipient). Are the three physical dimensions and the processes occurring therein made more comprehensible by the additions? Schrödinger's attempt in this *Journal*[1] to replace the 'particles' of physics by a multi-dimensional wave-function and Born's comments[2] on the expedient showed how precarious the claim to *Anschaulichkeit* may prove in the Hilbert space of quantum mechanics. Considering that geometry itself in any accepted sense may fail for certain problems in the nuclear domain, we may well doubt whether a multi-dimensional space will satisfy the needs of a still young and unsure science of psychology. All this is not to deny the *empirical* value of Smythies' researches into the mescaline hallucinations. My contention is that the phenomena afford insecure grounds for his speculation on multi-dimensional spaces.

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[1] S.W. Fernberger, 'Observations on Taking Peyote', *Amer. J. Psychol.*, 1923, 34, 267-270

[2] See my 'Note on Multi-dimensional Time' in this *Journal*, 1957, 8, 155

[3] H. Weyl, *Philosophy of Mathematics and Natural Science*, Princeton, 1947, p. 136

C. T. K. Chari

Reply to Professor R. O. Kapp

Professor Kapp has said[3] that my disagreement with his theory of Diathesis seems to be due to a misapprehension of what his theory is. As I am not clear as to the nature of the alleged misapprehension I shall begin by giving a short description of what I have understood to be the meaning of the peculiar technical terms he uses:

Diathete: an immaterial agency, which exercises powers of selection, guidance, and control in connection with processes occurring in physical objects located in space, although it is not itself located in space nor is it subject to the laws of physics.

Diathesis: a process, resulting from the operations of a diathete, manifesting characteristics of selection and control.

Diatheme: a material structure located in space time which is, at any rate in part, the product of a diathesis caused by a diathete.

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[1] E. Schrödinger, 'Are there Quantum Jumps?', I and II, this *Journal*, 1952, 3, 109-123; 233-242

[2] Max Born, 'The Interpretation of Quantum Mechanics', this *Journal*, 1953, 4, 95-106

[3] *This Journal*, 1957, 8, 159

It follows, according to Kapp's theory, that no 'lifeless machine' (i.e. a diatheme) left to itself, and operated merely by material causes in accordance with the laws of physics, without the assistance of a diathete, can manifest diathesis. That Kapp's theory does entail such a basic distinction between 'living' and 'lifeless' machines seems to me to be an obvious logical consequence of the theory. This basic distinction was the one to which I referred in the opening sentence of my paper on Diathesis and Photosynthesis,[1] which Kapp now says is 'incorrect'. I leave it to the reader to judge whether I have misapprehended Kapp's theory on this point. In particular I would refer the reader to Kapp's descriptions of the meanings of his technical terms given in his book *Science versus Materialism* on pages 262–264.

In my paper I suggested an analogy between the self-winding watch and photosynthesis. I was concerned to point out that certain lifeless mechanisms were as capable as 'living machines' of manifesting diathesis, merely through the operations of the laws of physics, and without the purposefully controlled intervention of a diathete (after the initial design and construction stages). My contention was :

> 'the self-winding watch affords a simple example of an inorganic mechanism which has all the essential characteristics which Kapp ascribes to a diathete [but] which is completely subject to the law of physics'

Kapp on the other hand says :

> 'the lifeless machine, be it an electronic computer, a toy tortoise, or a self-winding watch only does what it has been designed to do if another machine, *a living one in every case*, does something specific to it. The owner of the watch must, for instance, ensure that it be *properly* shaken on occasion. . . . For the watch to be wound there must be *selection* of the minimum intervals between the shaking, of the nature of the shaking, and of its minimum duration . . .' (my italics throughout).

In other words Kapp contends that no diatheme, such as a self-winding watch, if left to operate by itself, in accordance merely with material causes working according to the laws of physics, can manifest the typical kind of activity which he claims to be characteristic of a diathete 'in which a continuous and random movement of particles causes a specific effect intermittently and at specified moments of time'.

Now it was never my intention to deny that the design and construction of a complex lifeless machine (such as a watch) is a peculiar feat which is, at present, beyond the capacity of other lifeless machines without the intervention of a diathete, viz. a human mind. So I should agree with Kapp that 'diathesis is applied to the watch for its design [and] for its construction'. Where I differ fundamentally from Kapp, however, is that I reject his contention that diathesis *must continue to be applied by a diathete* after the watch has been designed and properly constructed, if it is to work satisfactorily. It is important to be quite clear what the acceptance of this contention of Kapp would entail in a concrete case of the working of a diatheme, such as a self-winding watch. It would mean, for example, that it would not be possible for a self-winding watch, which had previously been set by a diathete (a human experimenter), to wind itself and to keep time, if it were to be disconnected from the casual operations of a diathete and were to be left in contact with merely adiathetous structures. (Kapp says :[1] 'I describe what is neither a living nor a lifeless machine as an adiathetous structure' : so that the random waves produced by an irregular disturbance in a liquid would constitute an adiathetous structure.) Therefore, on Kapp's theory a self-winding watch should be unable to wind itself and keep time if it were tied to a float and left in a pond whose surface was disturbed by a wind. Yet anyone who cares to take the trouble to tie a high-grade waterproof self-winding watch, such as a Rolex Oyster, to a rubber ball and toss the ball into a pond where there are waves, will find that Kapp's theory is not in accordance with the empirical facts : I found that the watch wound itself and kept time for some time after the wind had died down.

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[1] *This Journal*, 1957, 8, 140

Kapp might attempt to reconcile these results with his theory, by ascribing the diathesis, thus displayed by the watch in an adiathetous environment, wholly to the *past* operations of diathetes which had taken part in the design and construction of the watch. But this will not do, because Kapp's theory needs to show that a *diathete is necessary* to wind the watch and keep it going (i.e. for its *operation* and not merely for its design and construction). For he says 'the watch does not really wind itself. Diathesis is applied to the watch for its design, for its construction and *for its operation*' (my italics). But where is the diathete which, on Kapp's view, must be present to apply the diathesis to wind up the watch and keep it going so long as the wind lasts in the first experiment described above? It cannot exist in either the wind or the water or the watch : because Kapp tells us a diathete is 'a mechanism that can only exist in a living substance, that has a very specific action'.[2] Nor will the answer that the diathete is the human experimenter do. For in the experiments described, it is *not* the case either that 'the owner of the watch must ensure that it is *properly* shaken on occasion', nor that 'there must be *selection* of the minimum intervals between the shaking, of the nature of the shaking, and of its minimum duration' (my italics). There is no selection in these experiments at all of the kind Kapp attributes to a diathete as contrasted with a lifeless material structure. Nor is it necessary that there should be. For in the case of the experiments described the diathesis follows simply from leaving the diatheme, the self-winding watch, alone to behave in accordance with well-known laws of physics (including the laws of statistical and quantum mechanics in that term), operating upon the initial conditions, which conditions are themselves made indeterminate by the act of tossing the watch into the water. To introduce a diathete into the explanation is not only unnecessary, but would be positively embarrassing, in view of Kapp's idea that a diathete cannot be located in space.

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[1] *This Journal*, 1957, 8, 159

[2] R. O. Kapp, *Mind, Life and Body*, London, 1951, p. 183

I conclude therefore that Kapp has failed to put forward a valid argument in reply to my contention : that certain material structures located in space, and operating wholly in accordance with well-known laws of physics, can display characteristics typical of what Kapp calls diathesis, without the continued intervention, operation, or control of an immaterial diathete. Kapp's theory is thus seen to be unnecessary in this connection ; and in my view it is also sterile since it gives no guide or encouragement towards the kind of experiments needed to elucidate the behaviour of ' living machines '. Indeed the theory seems positively to *discourage* such experiments : since it postulates the existence and causal efficacy of entities not located in space and not subject to the laws of physics. Thus Kapp's theory of diathesis seems to me to be unsuitable for adoption as a positive philosophy of science, though negatively I admit having got mental stimulation from reading Kapp's theory and reacting against it.

H. A. C. Dobbs

Professor Kapp replies :

Dr Dobbs seeks to prove that a watch will be wound up even if nothing is done to it by a living machine. To do so he points out that the watch will be wound up if something *is* done to it by a living machine, namely if such a machine, a man for instance, selects a suitable float, ties the watch to this, and selects a disturbed surface of water on to which to throw the diatheme that he has just constructed.

DISCUSSIONS

MEN AND MACHINES

1 *Introduction*

If we think about a machine, we ask '*What* is it ?' and '*For what* is it good (or made) ?' The same questions have been asked about man : 'Who is he ?' and 'For what is he good ?' (or, put in other words, 'For what purpose is he on the face of this earth ?'). As we know, for thousands of years, and in many quarters even today, religious cosmologies and myths provided the answers to these questions. With the growth of the physical and biological sciences, particularly during the nineteenth century, new answers for these questions were proposed. First, the question of man's 'identity' was recast from the mould of divine creation into a scientific model : the model was that of 'animal'. The new view of man was that he was like other animals ; he was merely a more highly developed form of mammal than any other in the animal kingdom.

This idea appears to the educated man of today as banal. We must remember, however, that until the days of Darwin (and even thereafter) the differences between men and animals puzzled people in much the same way as we now cogitate about and analyse the differences between men and machines. Thus, scientists asserted that men possessed 'consciousness', 'volition', and 'free will', whereas animals were supposed to lack these qualities. Similar arguments are advanced nowadays in attempts to document the differences between men and machines.

Clearly, the problem of man's self-feeling is to be reckoned with when he is likened to a machine. It might be best, therefore, to make this emotional bias explicit at the outset. Otherwise one is constantly handicapped by working against the *hope*—that is, of the reader, and perhaps also of the investigator himself—that man *should* turn out to be as much *unlike* the machine as possible. While this attitude is understandable, it is also undesirable if one wishes to push the similarity between man and machine just as far as it is profitable. In this connection we can draw on the lesson that we have learned from the likening of man to animal. The initial insult to what Freud called man's narcissism was, I think, more than compensated for by the enormous advances in physiology, pathology, bacteriology, biochemistry—and thus in medicine—which it made possible. It seems likely, therefore, that similar advances in psychiatry and the social sciences might be facilitated by the machine analogy. Those who are psycho-analytically inclined may note that the very 'repulsiveness' of the machine analogy—particularly for those who believe that they treasure human values (and who believes otherwise ?)—should be a warning. The strength of the affect mobilised might signal its very relevance to the topic at hand.

There is extensive literature on the use of machines as models for the brain, man, and behaviour. What justification can I claim for taking up additional space and time by adding to what has already been said by others?[1] My reasons for doing so are, briefly, twofold. First, I wish to call attention to the relevance of an object-relations theory of personality to the problem of by whom and how man is controlled. Secondly, the rôle of the 'body schema' in men and its relation to the machine analogy was discussed recently by Miles,[2] and I would like to offer some comments of my own on this subject. My conclusions on these two points are simple and I shall state them first and document them later. They are (*a*) that men are controlled by internal (and external) objects and (*b*) that machines have no 'body schema' because men design machines to gain 'practical' ends, whereas a machine with a 'body schema' would be one whose main job was to take care of its own 'body'. The 'body schema' is thus viewed not as a special human 'property' but as internal object, 'purpose', and 'value'.

2 *What is a Machine?*

At first glance this question poses no difficulty. We all *know* what a machine *is*. The images of cars, vacuum cleaners, and calculating machines appear before us, to document our conviction. These are all machines because they were all *built* by men. They are, as we say, *man-made*. This is a key concept and it is usually contrasted with that which occurs *naturally*. Immediately the going gets rougher. What is meant by something occurring 'naturally' is not very clear. Presumably it means that it is *not* man-made. This usage is illustrated by the adjectives applied to fibres. Nylon and orlon are 'artificial' or 'man-made'. Wool is a 'natural' fibre. It is not made by man. If one were pushed to answer *who* makes it, perhaps he would say it is made by a lamb. We shall see presently the trouble into which this line of thought leads us.

But first, let us turn our attention to another example. Take a sturdy stick of wood. It occurs 'naturally' and (therefore) is not considered a 'machine'. But suppose a man picks it up and uses it for a lever. Now it has become a 'machine'. One argument to help us find a way out of this dilemma runs as follows. What a machine *is* depends fundamentally on how man uses the object in question. In other words, it is the *purposive act* that is the distinguishing feature of this concept.

A similar argument, and one which I personally prefer, calls attention to the element of *intent*. This introduces a psychological and social dimension and highlights the interesting connections that may arise from analogies between men and machines. Intent and purpose are concepts for which it is necessary to assume self-awareness and communication. Thus, a stick of wood lying in the forest is not—and one may add, although this would be redundant, *socially*—a machine. A stick *used* (by someone) to move a heavy object, on the other hand, is a machine.

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[1] See in this connection particularly the works of the following investigators with whose views I am in substantial agreement. D. M. MacKay, 'On Comparing the Brain with Machines', *Amer. Scientist*, 1954, **42**, 261; 'Mindlike Behaviour in Artefacts', this *Journal*, 1951, **2**, 105; A. Rapoport, 'Technological Models of the Nervous System', *ETC.: A Review of General Semantics*, 1954, **11**, 272; W. Sluckin, *Minds and Machines*, Harmondsworth, Middlesex, 1954

[2] T. R. Miles, 'On the Difference between Men and Machines', this *Journal*, 1957, **7**, 281

This argument, if correct and acceptable, shifts the focus of attention away from the physical properties of the machine (i.e. whether it resembles the human body or not). It focuses, instead, on psychological and social considerations (i.e. the nature and purpose of the action or contemplated action).

If we return now to the example of different fibres, we can clarify the issue quite satisfactorily. We can say that the difference between nylon and wool is not described fruitfully by calling one 'artificial' and the other 'natural'. Operationally, both are made by man, one by chemical synthesis, the other by animal husbandry. Similarly, we have the examples of 'natural' versus 'artificial' diamonds or of 'natural' versus 'synthetic' penicillin. In these examples it is clear that from the point of view of physics, there is no difference between the two categories. The difference lies in their *history* and this is a *socio-psychological datum*.[1] Thus, the difference between two diamonds may be *solely* that one comes from a mine in South Africa and the other from a laboratory in Schenectady. This difference, we should recall, is of the same *type* as the difference between two other diamonds, when one comes from mine *A* and the other from mine *B*, both in South Africa, albeit the former difference may seem much more *interesting* to us, at least at this time.

3 *Intent, Purpose, and Control*

If the word and concept 'machine' is used to refer to the manner of functioning rather than the physical make-up of the object, we can by-pass (at least for the time being) the problem of (static) physical similarities and differences between men and machines. As we have noted, machines are used with intent and purpose. They are thus *controlled*. They may be controlled, moreover, by men, animals or even by other machines. Machines have, as Kapp [2] has put it, 'input functions'; they are built for a purpose and are controlled by an operator. I will now try to show that these characteristics can be demonstrated in men also. At the risk of seeming pedantic, I repeat that this does not mean that I assert that 'Men *are* machines'. It only means that the difference between men and machines may be of an order of magnitude similar to that between men and animals. This view may be stated more accurately, perhaps, by saying that it is advantageous to consider men, machines, and (lower) animals as belonging to the same category. There are, of course, significant differences between them. This should, however, not obscure the important distinctions between all members of this category and things that are not in this category.

I cannot document my thesis within the space limits of an article. Nor perhaps is it necessary to do so here, since to the readers of this Journal the views that follow will not be completely novel, if they are novel at all.

I shall begin, again, with what might be my conclusion. We call machines 'machines' because it is apparent to us that they are being controlled. We think men are *not* machines because it seems to us that they are not being controlled. We express this by saying that they are 'free' or have 'free will'. My thesis is that men are 'free' to the extent to which we are ignorant of the forces that control them. And, indeed, when men are *obviously* controlled, we then readily think of them as animals or machines.

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[1] B. Russell, *Human Knowledge, Its Scope and Limits*, New York, 1948, pp. 44-53

[2] R. O. Kapp, 'Living and Lifeless Machines', this *Journal*, 1954, 5, 91

There are two basic questions to be answered in connection with this thesis. They are : 'Who controls men ?' and 'How are they controlled ?' The more fully these two questions could be answered, the more meaningless (and uninteresting) the controversy about the differences between men and machines would become. This, at least, is my impression.

When I ask, 'Who controls men ?', I do not have in mind the crude examples of political control, such as those of Hitler or Stalin. We must begin, albeit arbitrarily, at some point of 'origin' and build on it. In this we follow the pattern of the construction of a machine ; the beginning of this process is fixed, although arbitrarily, at some point in the history of the raw materials. The materials are brought together, let us say, in an automobile factory, and are manufactured into the finished product. It is almost always possible to keep going back to ever earlier patterns of construction, since the 'construction' of the raw materials themselves can be analysed in a similar manner. In the human being, it is convenient to start at the moment of birth. The newborn infant is the 'raw material' from which the adult man is made. The way in which he is 'made' goes a long way in explaining who controls him and how. Actually, we know a great deal about how his physical body is made and no scientist today objects to looking at the body as a machine. It is a physico-chemical machine, manufactured out of the original raw materials and the substances that are added to it, in the form of the necessary food-stuffs, vitamins, minerals, etc. As I have suggested elsewhere [1]—and, indeed, as it is inherent in an object relationship theory of the personality [2]—the 'person' is built from 'objects', much as the 'body' is built from food. The implications of this process of object-internalisation are far-reaching. Objects carry with them all the things which we designate by such words as 'goals', 'values', 'skills' and so forth. Thus, what a human being as a 'machine' can do will depend, on the one hand, on the physical plant (the body), and on the other hand, on the psychological plant (the person). (This usage, incidentally, may lead us to revise our views about the concept 'ghost in the machine'.[3]) Internal objects, however, not only 'make up' the ego, but, in some measure, also *control* the 'man'. This may be visualised as a historical sequel to the parents' control of the child. In addition to the potentially very numerous internal objects, man is also controlled by external objects, in the form of other people, social rules, physical conditions, and so forth. I submit that it is this great complexity of the controlling forces which obscures the fact that man is controlled and gives rise to the impression of random (or free) activities. It is as if a long and complicated menu had been prepared by innumerable cooks, some cooking one dish and others another, while still others added a little seasoning here and there ; and, imagine further that all this is being done without any central plan or supervision of the meal. If, when the meal was finally served (and this would correspond to an *adult* man living in society), we were to ask who cooked it, how would we answer it ? No-one can be named ; no-one can be held 'responsible'. Would we conclude that it is not a 'meal' ('machine') at all ? Perhaps so. This is what we do, it seems to me, when we say that man is fundamentally unlike a machine, because we find it impossible to describe (with reasonable brevity) who controls him and how.

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[1] T. S. Szasz, 'A Contribution to the Psychology of Schizophrenia', *Arch. Neurol. Psychiat., Chicago*, 1957, 77

[2] W. R. D. Fairbairn, *Psychoanalytic Studies of the Personality*, London, 1952

[3] G. Ryle, *The Concept of Mind*, London, 1949, pp. 15-16

I should add here, by way of explanation and apology, that I have used the word ‘ control ’ rather loosely in this discussion, sometimes to refer to mere ‘ influences ’, at other times to designate situations in which the subject has hardly any choice at all in his adaptation to the ‘ controlling force ’. This distinction, of course, is important and leads to further problems. Considerations arising from this source, however, will not be discussed in this paper.

4 *Men, Machines, and the ‘ Body ’*

I have thus far, while adhering to a psychological point of view, concentrated on pushing the similarities between men and machines as far as possible. Recently, Miles [1] suggested that men seem unlike machines *most specifically* in that they have a ‘ body schema ’ and machines do not. If this is the last line of defence of those who wish to emphasise the differences between men and machines, their position seems to me practically hopeless. My reasons for this statement will be set forth briefly. They are stated, however, more fully in a recently published book [2] which is devoted almost entirely to a consideration of the nature of the relationship between *ego* and *body* (which Miles, and many others, call ‘ body schema ’). In fact, it is my interest in this subject which has stimulated me to bring together here some interesting connections between ego-body relationships on the one hand, and the problem of men-and-machines, on the other. Prior to Miles's recent contribution, the connection between these two topics was, apparently, not considered particularly pertinent. I would like to state an opinion somewhat at variance with that of Miles. Furthermore, I wish to bring my comments into connection with the nature of object relationships, a subject whose importance also has been recently brought to the attention of the readers of this *Journal*.[3]

I shall not discuss my objections to some of Miles's reasoning in connection with the construction of a ‘ homo mechanisma ’. He speaks of such a machine being made ‘ artificially by a sufficiently competent engineer ’.[4] The word ‘ artificial ’ is important in Miles's argument ; I have commented earlier on the difficulties which arise out of the dichotomy of ‘ artificial-natural ’. I would maintain that man—any man—is largely ‘ artificially ’ produced, if by this word we designated, as we usually do, the effects of human intervention. In fact, the modern term ‘ human engineering ’ implies awareness of this fact.

The crux of Miles's argument, and the only major issue to which I propose to address myself now, is contained in these (his concluding) lines :

> ‘ Men can relate their perceptions to a body-schema, but in the case of machines there is no body-schema to which perceptions could be related.’ [5]

[1] Miles, op. cit. pp. 283-292

[2] T. S. Szasz, *Pain and Pleasure, A Study of Bodily Feelings*, New York, 1957

[3] W. R. D. Fairbairn, ‘ A Critical Evaluation of Certain Basic Psycho-Analytical Conceptions ’, this *Journal*, 1956, 7, 49; and the discussion of this paper by M. Balint, S. H. Foulkes, and J. D. Sutherland, together with Fairbairn's reply, this *Journal*, 1957, 7, 323-338

[4] Miles, op. cit. p. 278

[5] Miles, op. cit. p. 292

In attempting to deal with the problem of pain and certain other bodily feelings, from a psychological point of view, I was struck by the difficulties which one encounters.[1] The traditional psycho-analytic framework seemed unsatisfactory. So did, at least to me, the concept of body-schema, originally developed by Head,[2] and greatly expanded by Schilder [3] (as the body-image). I have then re-interpreted various data previously dealt with in these terms by viewing the ' body ' as an object *vis-à-vis* the ego. The term ' body ', in the foregoing sentence, does not refer to the subject's ' physical body ' or to someone else's view of his body. It refers to the ego's perception and conception of the body. This usage, as will be evident, is the same as that employed in psycho-analysis with regard to external and internal objects. In other words, when we speak of a patient's mother, we mean *his* image of the mother (and not the analyst's or someone else's). There may be several such images, and similarly, there may be several ' ego-body integrations ', some closer to consciousness than others, and some more in conformity with the socially validated images of the patient's body than others. Suffice it to say, in this connection, that I have considered the body to constitute one of the most important objects in the ego's orbit. I have suggested, also, that the body is historically the *earliest* object and thus of particular importance in severe psychopathological states which are characterised by a loss of personal objects and by severe regressions.[4]

What does this have to do with the differences between men and machines ? Miles suggested that men are uniquely different from machines because men are oriented (as I would put it) toward their bodies. But, I would object by pointing out that this is precisely how ' man-as-machine ' has been put together. It is a characteristic of the human machine that it wants to keep the machine on the go. All machines have a purpose. They are built to do something. What is man built to do ? In modern times, with the fall of religious ideals, it has not been possible for intelligent persons to agree about the purpose of the ' machine '. But in entertaining such ' higher ' purposes, the machine's basic purpose—which was enunciated in a way by Claude Bernard a century ago—was overlooked. This is to keep itself going, to keep the machine intact. In this light, the last (if it is the last) difference between men and machines falls away. Perhaps no machine has a ' body-schema ' because none has ever been made in such a way that it has no useful purpose *for man*—but is so constructed that its chief aim is to maintain itself. I have not the faintest idea whether anything like this could be constructed. But that is not the point. The point is that if we wish to build a machine that will imitate *man*—and not some of his special *skills* such as lifting weights or counting—then we would have to build a machine in such a way that it would be oriented to the maintenance of its own physical equipment.

5 Epilogue : A Problem of Values

I have tried to show that one of the most interesting parameters around which the difference between men and machines may be organised is that of ' purpose '

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[1] Szasz, op. cit. Chapter 5

[2] H. Head, *Studies in Neurology*, London, 1920, Vol. II, pp. 605-608, 722-726

[3] P. Schilder, *The Image and Appearance of the Human Body*, New York, 1951

[4] Szasz, op. cit. Chapter 8

(and ' function '). All machines are *good* for a certain purpose. Some lift weights, others take us from place to place and still others are used for making various things. If we look upon man as a machine (and even if we do not), we must answer the question of man's ' purpose '. For what is he good? This question plunges us into a morass in which many scientific problems have foundered : we face a problem of *values*.

Actually, we must realise that the problem of man's purpose is quite recent in the history of civilisation. To large numbers of men in many parts of the world, it does not constitute a ' problem ' even today. The men in Plato's *Republic* each had their discrete functions. Indeed, it is characteristic of all *Utopias* for men to have clear-cut niches in the scheme of things. There is no *anomie*,[1] and there are, therefore, no existentialists. It seems to be a specific characteristic of our age that relatively large numbers of people have come to realise that none of the previously accepted goals of life are 'necessarily' theirs. To live ' for God ' or ' for Country ' may have made our ancestors *feel* like ' free men '. Many people nowadays, however, feel that such goals characterise a ' robot ', or a ' slave to a cause '. The titles of two of Fromm's books aptly characterise this mood : *Escape from Freedom* and *Man for Himself*.[2] Although rather shallow in philosophical penetration, these books illustrate the problem of values that must be considered in connection with any far-reaching analogy between men and machines.

To sum up, it seems to me that the intellectual of today resents the notion of his being good for any *one* thing. He feels any single purpose, skill, or function in life, or just a few of these, as too confining. It no longer suffices for most men to feel that they may become skilled at say boxing, mathematics, or playing the piano. In so far as they confine themselves to any one, or a few, activities, they feel—and are made to feel—a *loss*. They lose, as it were, all the things which other men can do. If we were to judge machines in this way, we would decry the workings of automobiles because they cannot fly, and of flying machines because they cannot harvest wheat. We accept the fact that machines are useful for only one, or at best a limited number, of tasks. Similar considerations, when applied to man, now run afoul of sentimental and moral consideration. Clarification in this area is needed and must come from psychology (psychiatry) and philosophy alike. In psychiatry there is an increasing interest in the problem of so-called *ego identity*.[3] In machine-terms, this simply means a specific scrutiny of the make-up and purposes of the particular ' machine ' (man). How is he put together and what is he made to do? Some workers interested in the psychology of delinquency have gone so far as to suggest that youngsters who behave in a delinquent manner are ' made ' by their parents for this specific purpose.[4] These considerations are pertinent also in connection with the nature of psychotherapy. Several recent literary works [1] have dealt with this very subject—that is, with the questions ' What is man ? ', ' What does he want to be ? ' and ' What do others want him to be ? '—in a manner at once brilliant and provocative. Pushing the analogy between man and machine still further might enable us to bring scientific clarification to an area hitherto illuminated only by artistic insight.

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[1] See in this connection E. Durkheim, *Suicide*, Glencoe, Ill., 1951 ; and S. De-Grazia, *The Political Community*, Chicago, 1948

[2] E. Fromm, *Escape from Freedom*, New York, 1941 ; *Man for Himself*, New York, 1947

[3] E. H. Erikson, ' The Problem of Ego Identity ', *J. Amer. Psychoanalyt. Assoc.*, 1956, **4**, 56

[4] See for example A. M. Johnson and S. A. Szurek, ' The Genesis of Antisocial Acting out in Children and Adults ', *Psychoanalyt. Quart.*, 1952, **21**, 323

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INDUCTION AND NON-INSTANTIAL HYPOTHESIS

(A Review of Dr Wisdom's Views)

I

SOME contemporary logicians maintain that all reasoning is deductive in character. Others do not go so far as to identify reasoning and deduction but all the same invent arguments to undermine the importance of induction in science. Their disparagement of inductive reasoning is based, among other things, on their belief that induction has no rôle—at least not any significant rôle—in scientific investigation. In this paper I shall take Dr J. O. Wisdom as the representative of the above kind of anti-inductive approach. Dr Wisdom elaborates his thesis in his study on the *Foundations of Inference in Natural Science*, and I shall, in this paper, confine myself as far as possible to Dr Wisdom's thesis as expounded in this work. Dr Wisdom thinks that inference in natural science—traditionally supposed to be inductive—contains little of what we generally mean by inductive inference. He says that ' induction plays no part whatever in science—that there is no inductive method and that nothing approximating to inductive inference is used '.[2]

Dr Wisdom deplores that in the past traditional logicians failed to draw the important distinction between ' induction as a form of inference ' and ' induction as a method '. But, he says, this important distinction must not be overlooked. Why ? The reason offered by Dr Wisdom is an appeal to scientific practice. According to him, ' it is an important distinction *because* the practice of science makes it clear that whether or not there is a pattern of inference, there is no method of induction '. Apart from such appeal to scientific practice, Dr Wisdom, it appears, nowhere in the book referred to, explains the distinction. We shall not find it possible in this paper to consider whether the statement that there is no inductive method is tenable or not But for the present it may be stated that apparently by inductive method Dr Wisdom means a method of a certain character, a method that satisfies some conditions recommended by him (but what these recommendatory conditions are Dr Wisdom nowhere mentions), a method somehow distinguished from the ' so called ' inductive methods that are, according to him, either not inductive or too simple to deserve the illustrious name ' method '.

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[1] N. Dennis, *Cards of Identity*, London, 1955; B. Russell, *Satan in the Suburbs and Other Stories*, New York, 1953, pp. 1-59, and *Nightmares of Eminent Persons and Other Stories*, London, 1954, pp. 21-30

[2] J. O. Wisdom, *Foundations of Inference in Natural Science*, London, 1952, p. vii. All the citations from this book, in this paper, are from Chapters I and III except where the citations are referred to in the footnotes.

As to inference, Dr Wisdom argues that whatever may be said about generalisation, there is no doubt that a non-instantial hypothesis cannot be assimilated by inductive inference. Dr Wisdom, therefore, introduces a sharp distinction between ' generalisation ' and ' non-instantial hypothesis '. We shall first of all explain these terms and then consider the relation between them.

2

Induction is generally conceived as a process of the attainment of universal propositions from particular propositions of perception. The process as well as the universal conclusion attained are generally referred to by ' generalisation '. What is important from the point of view of Dr Wisdom is that generalisation is a universal proposition drawn from *instances* of which it is a generalisation. The above account, we shall see, unwarrantedly restricts the scope of inductive reasoning—the source of generalisation. The account is narrow, of course, not because it emphasises the instantial character of induction, but simply because of the fact that there are inductive processes that are not covered by the above definition.

The word generalisation is, we have seen, used in two different senses. Let us agree to use it now in the sense of universal *proposition* as distinguished from the *process* that results in such empirical propositions. Even then it is not unambiguous, for in this restricted sense it no doubt indicates empirical universal propositions, but it refers to universal propositions that result from a variety of processes. For example, generalisation is the conclusion of (*a*) inference from some to all of a species, of (*b*) inference from some species to all species of a genus. The universality in (*a*) is specific and comprehends only similar instances which differ not conceptually but only spatio-temporally, e.g. iron/oxygen is. . . . In such cases the subject concept is what the schoolmen called *infima species*. Such universal propositions are sometimes called, ' specific laws '. But the universality in (*b*) is generic since the subject concept is a generic one, e.g. all metals/elements are. . . . In such cases the universal comprehends specific instances which differ from one another conceptually and meet in a more general concept. The process which yields such universality is sometimes called ' generalising ' and hence the conclusion simply a *generalisation* as distinct from *specific laws*. Now, if (*b*) is to be regarded as an independent and self-complete process, then the word instance in the above-noted definition of induction is to be interpreted a little liberally. For otherwise (*b*) does not fit in with the above definition of induction, i.e. it is not strictly speaking true of (*b*) that it is a generalisation from instances. For it is really a generalisation about kinds or species, e.g. of metals; and species are concepts as distinct from instances, which are, in the given example, individual pieces of gold, silver, etc. In (*b*) we have already interpreted ' instance ' liberally and accordingly used the expression ' specific instances '. In this paper we shall make a plea for a more generous interpretation of the word instance. That is, we shall treat concepts also as instances in relation to concepts of a higher order. We shall try to carry this difference in instantiation to its limit.

Non-instantial hypotheses are universal propositions of which the concepts are non-instantial or, to be more precise, of which not all the concepts are instantial. That is, while all the concepts of a generalisation have exemplification in particular instances, in a non-instantial hypothesis there is at least one concept that has no observable instances.

This position naturally brings in the question of origin of ideas discussed by the British empiricists. This fundamental question will not be discussed in this paper, but we shall accept the general conclusions of the empiricist philosophers. We shall try to show that the concepts cited by Dr Wisdom as non-instantial are really concepts derived from instances. Also, an attempt will be made to trace the source of Dr Wisdom's confusions—his presuppositions which resulted in a false conclusion. Lastly, we shall try to establish the general statement that no concept used in science is non-instantial—that all such concepts have some empirical basis and are inductively arrived at by the abstractive activity of the mind.

3

We have long deferred the consideration of non-instantial hypotheses cited by Dr Wisdom. Dr Wisdom elaborated practically a single instance—' a most instructive example of the *formation* of non-instantial hypotheses '—Newton's Second Law of Motion, which states that the rate of change of momentum of a body is proportional to the force acting on it in the direction along which the momentum changes or, more simply, force is proportional to acceleration (when mass is constant). This law may be derived from experiments with an apparatus called Fletcher's Trolley. The experiment is described in detail by Dr Wisdom in Chapter III of his work under review. I describe below the experiment as far as possible in Dr Wisdom's words.

Fletcher's Trolley consists of a weighted trolley moving over a horizontal surface. It is drawn by a falling weight attached to it with a thread that passes over a pulley. A piece of vibrating steel traces waves on a paper placed on the moving trolley. From the known time period of the vibrating steel, time intervals (t) corresponding to different distances (s) travelled by the moving trolley can be found out. From these data acceleration (f) of the trolley may be calculated.

Now the experiment is conducted in three stages: (1) P (the pull of the falling weight) is constant. W—the total weight of the trolley is also constant. Measuring s and t we find that s/t^2 is a constant; i.e. acceleration due to a falling weight is uniform. (2) In the second phase of the experiment W is successively increased. Comparing the weights W moved by P and the acceleration with which it moves we find that f (acceleration) is proportional to $1/W$, i.e. fW is a constant. (3) In the last phase W is held constant and P is varied. Comparing P and f we find that f is proportional to P. Now, since f is proportional to $1/W$ when P is constant (the result of the second phase) and f is proportional to P when W is constant (the result of the third phase of the experiment) it follows that f is proportional to their product—P/W, when P and W vary.

Now that the experiment is concluded and the hypothesis formulated the question arises, according to Dr Wisdom: how to test the hypothesis? Dr Wisdom says that the difficulty of testing it, i.e. connecting it with perception, is due to the fact that of the ' entities involved '—P, W, f, etc., P is non-instantial. We shall presently enquire into the difficulties, but it will be profitable to compare the above ' non-instantial hypothesis ' with a law cited by Dr Wisdom as instantial—Charles's Law for gases according to which the volume v of a fixed mass of gas at a constant pressure p is proportional to the absolute temperature T. This law is instantial because v, p, T are all instantial, i.e. measurable quantities.

I have the following comments to make on the above example of non-instantial hypotheses:

(i) If Charles's Law is instantial it is not easy to see how Newton's second Law of Motion can be regarded as non-instantial. For the p in Charles's Law implies force, because p is nothing but force (F) divided by area (A), i.e. F/A. So if p is instantial, how can force be said to be non-instantial? In describing ' the experiment no. X (the Boyle-Mariotte Law) ' with a J-shaped tube in which a volume of gas is trapped in the closed end with mercury in the rest of the tube, Dr Wisdom says ' the long part of the J may be moved up and down thus altering the levels of mercury; this alters the pressure on the gas. . . . The difference between the two mercury levels gives the mercury pressure '. Is pressure in this case an observable instance? The difference between two mercury levels is indicative of pressure only when it is already presupposed. Again, Dr Wisdom says that while Newton's second Law of motion is non-instantial Charles's Law is instantial for ' if an apparatus is set up in which the volume, pressure and temperature may all be varied, instances of the law may be observed '. But in the experiment with Fletcher's Trolley (described above) too, the ' entities ' W, and P (the motive force) are all varied (we may ignore the case of f—acceleration—which according to Dr Wisdom is an observable kind). It should, then, follow that the law of motion referred to is an instantial law. Dr Wisdom admits that ' we feel force (as we have the feeling of pressure) when a body falls on us ' but yet force is a non-instantial concept, for ' this (our feeling of force) would entitle us to attribute force to the body only if it were thrown at us'. If this were a sufficient reason for not regarding force as instantial, weight too is to be considered non-instantial, for we experience ' the feeling of pressure ' only when ' we support something '.

(ii) Newton's second law of motion is attained, in the above example, experimentally, and in the experiment nothing non-instantial is, and, in fact, can be, used. Dr Wisdom himself in describing the experiment admits that force is a measurable quantity. ' Force ', he says, ' is measured by means of weights '. He also says that force ' is simply the pull of the weight attached to the thread '; ' let the motive force be denoted by P '; and also in describing the experimental set-up he says that to overcome the friction of the table-surface that may interfere with the motion of the trolley the table should be slightly tilted towards the trolley so that ' the trolley will remain at rest unless some force is applied ', so that, ' the apparatus is subject to no forces '. It is strange that after accepting force as a fact during the experiment Dr Wisdom denies, after the completion of the experiment, the factual character of force. If it is not anything observable or measurable what was the sense of measuring it during the experiment? It cannot be said that *motive force P* is different from *force*. For Dr Wisdom equates them; he says that ' with *motive force P* matters are different.

We know the experience of weights and can observe bodies fall; but *force* we do not perceive at all.' So it is clear that Dr Wisdom uses '*motive* force' to mean what is meant by 'force'.

(iii) After the completion of the experiment Dr Wisdom asks: how to test the hypothesis that force is proportional to acceleration? The answer is simple—by repeating the experiment. The law is the result of the experiment; in that sense it is already a verified one.

(iv) Dr Wisdom's use of the word hypothesis is ambiguous, he uses it sometimes to mean unverified guesswork and sometimes to mean any empirical generalisation that cannot be deductively proved. Newton's second law of motion is a hypothesis in the second, but not in the first sense. So the question how to test it does not arise; the experiment that yielded the law is its verification.

(v) Then, a distinction must be drawn between an instance and what is *considered* an instance. For an extreme subjective idealist, for example, there is no instance of matter, but commonsense and science regard it as instantial. When Dr Johnson claimed to refute Berkeley's idealism by kicking a piece of stone, he certainly considered a piece of stone to be an instance of matter. Similarly, concepts like substance, thing, causality, etc. are regarded in science as instantial though they may not stand the positivist or phenomenalist analysis. For example Dr Wisdom regards *thing* as an instantial concept, but Russell believes that 'we can give instances of associated sense data, but we cannot give instances of actual physical objects'.[1] Dr Wisdom seems to accept the Berkeleian analysis with regard to force, but the realistic or scientific approach with regard to pressure. If Dr Wisdom's contention about force is correct, the inescapable conclusion that follows is that pressure too is non-instantial. 'We are familiar with it (weight)', it is said, 'as the *feeling* of pressure when we support something'. But pressure, as a concept of physics, is not the feeling of pressure. Now, force is *regarded* in science as a measurable, i.e. observable quantity. The best thing I can do here is to appeal to Newton and authorities on Newton. Newton himself said of his theory of gravitation that 'in this philosophy particular propositions are inferred from the phenomena and afterwards rendered general by induction . . . and to us it is enough that gravity does really exist'.[2] Clerk Maxwell said that 'there is no formula in applied mathematics more consistent with nature than the formula of attractions'.[3] Speaking of the law of gravitation Whitehead observes that the law is 'purely based upon description of observed facts. . . . Newton himself insisted upon this very point. He was not speculating, he was not explaining. Whatever your cosmological doctrines may be, the motions of the planets and the fall of the stones, so far as they have been directly measured, conform to his Law. He is enunciating a formula which expresses observed correlation of observed facts.'[4]

After disposing of the instance of non-instantial hypotheses given by Dr Wisdom I propose to make some general observations on the anti-empirical approach to science, specially illustrated by Dr Wisdom's 'metascience'.

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[1] Bertrand Russell, *Problems of Philosophy*, London, 1956, p. 108; cf. P. W. Bridgman, *The Nature of Physical Theory*, New York, 1936, Chap. III, p. 18

[2] Newton, *Principia II*, London, 1803, p. 314

[3] J. C. Maxwell, *Collected Scientific Papers*, Cambridge, 1890, Vol. I, p. 157

[4] A. N. Whitehead, *Adventure of Ideas*, Cambridge, 1939, Chap. VII, § vii

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Thinking as such involves abstraction; it develops by a progressive increase in the abstractive activity of the mind. Abstraction is the precondition of theoretical activity. The more theoretical a science becomes the more abstract it is, and the more abstract, the better fitted it is to deal with concrete facts. Now, corresponding to different levels of thought there are grades of abstraction to which again correspond different degrees of instantiation. The word ' the ', for example, printed ten times exactly alike are various instances (called ' tokens ') of the class (or ' type ') word *the*. The token-type distinction, then, represents one type of instantiation. Then, the instantiation of the ' determinate ' and that of the ' determinable ' are of two different grades, e.g. the instances of this specific shade of red (the determinate) and those of red or of colour (the determinable). Consider, then, the relation of what we call things to the associated sense-data. Even in the case of what Mill calls ' natural kinds ' the relation between the instances and the concept is at best one of partial representation. But when we arrive at ' artificial kinds ' like ' edible things ' representation of concept has become fainter, because edibility is not substantive or attributive, but an operational concept. When, however, we reach complex relational concepts like cheating, organism, society, ordinal numbers, etc., the observation of the instances no longer consists in sense-experience. In this way, by a progressive abstraction, we arrive at concepts and instantiation thereof at different levels of thought. Now, Dr Wisdom seems to ignore this difference in instantiation.

Some methodologists (and Dr Wisdom seems to follow them) suppose that abstractions are fictions. H. Vaihinger,[1] for example, devotes a whole book to the classification and description of ' fictions ' used in various sciences—e.g. neglective, schematic, symbolic, summational fictions. But it must be remembered that abstractions are always abstractions from the particular, the concrete; they represent parts, phases, relations and processes of reality. Even a professed fictionalist like Vaihinger has to admit that ' the *material of sensation* is remodelled, recoined, compressed, is purged of the dross ' by the fictive activity which ' under the necessity, stimulated by the outer world, *discovers* the store of contrivances ', i.e. fictions. Scientific abstractions may be shown to be connected with some experience, to have arisen out of experience. Not even the ' abstractions ' of pure mathematics are mere abstractions. ' Mathematics ', it is rightly said, ' is as empirical as metallurgy. Men began with counting and measuring things as they began with pounding and burning them. The present-day mathematical logician may present the structure of mathematics as if it had sprung all at once from the brain of a Zeus. . . . But, nevertheless, this very structure is a product of long historic growth in which all kinds of experiments have been tried, . . . a history in which matter and methods have been constantly selected and worked over on the basis of empirical success and failure.[2]

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[1] H. Vaihinger, *The Philosophy of 'As If '*, London, 1924. A description of various ' fictions ' will be found in Part I. The expressions quoted are from Introduction : Chapters I and IV.

[2] J. Dewey, *Reconstruction in Philosophy*, New York, 1954, p. 116. Cf. Bridgman, op. cit. Chap. V, p. 52: Mathematics thus appears to be ultimately just as truly an empirical science as physics or chemistry, and the feeling that it is something essentially different arises only when we do not carry our analysis far enough.

Even the formalists who try to demonstrate that mathematical constructs are symbols, that the data of mathematics are ' arbitrary sounds or marks called symbols ', are constrained to admit that ' something in reality answers to them (symbols) '.[1] Irrationals, $\sqrt{2}$, $\sqrt{3}$, etc., for example, are numbers not in any extraordinary, irrational way. Like inductive numbers they satisfy the rules of addition etc. and the laws like the commutative, associative, etc. For instance, $\sqrt{2}$ may be defined as the ideal limit of the series of rational numbers whose squares are less than 2. And every student of mathematics knows that Pythagoras arrived at the concept of $\sqrt{2}$ in his attempt to solve a concrete problem and he used it to represent a concrete fact—the relation of the two sides of a square to its diagonal; the diagonal of a square of which the sides are of unit length is an instance of $\sqrt{2}$.[2]

Another reason for Dr Wisdom's supposition that concepts like force, attraction, etc., are non-instantial is his unconscious acceptance of the Aristotelian theory that what is ultimately real is the individual substance, entity, or thing. Now, emphasis on the primacy of individual substance is bound to undermine the importance of universals, especially of relational, situational, and operational concepts and consequently, of the abstract sciences; and who does not know that all modern sciences worth the name are highly abstract ? Dr Wisdom, we have already noted, accepts the above naïve account of thought and reality. In connection with Schrödinger's wave function he expressly admits it when he says that ' it denotes no kind of *entity*, it has no instances '. But modern science has long outgrown the classificatory zoology—the inspiration of Aristotle's theory of Primary substance—of fixed genus and species. The Newtonian physics also may be regarded, in a sense, as a legacy of the Aristotelian doctrine of substance; it is based upon the supposition that each bit of matter is a piece of individuality. This doctrine of ' simple location ', as it is called, has been abandoned by modern physics. Physical things are now conceived of as the ' modifications of conditions within space-time extending throughout its whole range '.[3] Dr Wisdom's search for substantives or ' observable instances ' shows that he clings to the Newtonian physics as to the Aristotelian metaphysics. But with the denial of ' simple location ' and the elimination of the substantive, observation in science has become very indirect. We do not observe entities or even facts. ' The sense-data on which the propositions of modern science rest are, for the most part, little photographic or inky curved lines on paper . . . we really see only the fluctuations of a tiny arrow, the trailing path of a stylus or the appearance of a speck of light.'[4] Dr Wisdom's observation, however, is the direct ' seeing ' of a ' verifiable '—an instance of thinghood.

If the concepts of science were fictional then no useful purpose could be served by them, and from non-instantial hypotheses nothing observable could be deduced. In the words of Whitehead, ' there is no valid inference from mere possibility to matter of fact, . . . from mere mathematics to concrete nature'. There must be, then, in reality something, however complex, that corresponds to the abstractions of science. Dr Wisdom seems to admit that when he says that to deduce a 'conclusion of an observable kind', 'certain known facts' are to be 'combined' to non-instantial concepts. But it is difficult to see how a known fact can be combined with a mere abstraction leaving the abstraction a mere abstraction. In a syllogism, for example, with the universal major which is interpreted non-existentially (to avoid the difficulty of *petitio principii*) an existentially quantified proposition is combined; but that amounts to the denial of the non-existential character of the major.

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[1] S. K. Langer, *Philosophy in a New Key*, Harvard U.P., 1942, p. 14

[2] Cf. ' Euclid certainly meant his axioms to describe the space in which we live and move. . . . The world of pure mathematics . . . has an appearance of unnatural smoothness and tidiness . . . yet the laws of geometry and mechanics came out of the study of the world and return to it in the form of applied mathematics.' C. D. Broad, *Scientific Thought* London, 1923, pp. 28, 50

[3] A. N. Whitehead, op. cit. Chap. IX, § vii [4] S. K. Langer, op. cit. p. 51

The most important condition of the legitimacy of hypothesis is that hypothesis must be capable of deductive development so that it may be shown to be connected with something verifiable. In discussing the question of verifiability or 'testability' Dr Wisdom rightly makes a distinction between 'transcendental judgment' and hypothesis. 'The collective mind for instance', Dr Wisdom says, 'is not a hypothesis, because from it no deduction can be obtained of a perceptual kind.' But Dr Wisdom does not enquire why the most abstract concepts of mathematics and physics, for example, are capable of deductive development but transcendentals, like collective mind, are not. If he enquired into the reason of this difference, he would find that while transcendentals are incompatible combinations of basic concepts, the abstractions of science are derived from a very complex experience—generally indescribable—of facts, processes and relations. That is why, while abstractions of science are ultimately cashable in terms of what are observable, the pseudo-concepts are not. The distinction between *abstract* and *transcendental* is important. Transcendental concepts are pseudo-concepts, they have no observable instances; and the 'hypotheses' in which they occur are not capable of deductive development, i.e. they are not hypotheses proper. But an abstract concept, however unrelated to any relevant experience it may appear, is traceable to observable instances; and the hypotheses in which all the concepts are abstract, i.e. concepts proper, are empirical in origin.

Dr Wisdom argues that concepts like force, attraction, etc. are arrived at by 'some mental jump', 'by some imaginative means'.[1] In case of generalisation we have some foundation—the instances—wherefrom the scientists jump; but what about non-instantial hypotheses? Where is the foothold from which the scientists will jump? There must be some experiential basis for the 'jump' into a non-instantial hypothesis. Dr Wisdom seems to ignore this and reintroduce into psychology the old concept of spontaneous generation. As regards the mental jump Dr Wisdom finds no difference between non-instantial hypothesis and generalisation, for 'the method of science is for the scientists to jump, by some imaginative means, to a generalisation or non-instantial hypothesis'.[2] From this he deduces the conclusion that 'there is no such thing as method of induction, for there is no machinery by which a general conclusion can be inferred from particular premisses in induction'. Is there any machinery, one may ask, by means of which one may jump to a hypothesis? Moreover 'imaginative means' presupposes some basis in sense-experience. Even the 'one contradictory phenomenon'[1] of Hume—the case of a particular shade of colour never experienced by sense but capable as it were of spontaneous generation in the mind—is empirical in origin, in the sense that it can arise in the mind only when the colours of which the shade is a mixture and the whole achromatic series of brightness, at least that particular shade in respect of other colours, are given in sensation.

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[1] Wisdom, op. cit. Chapters V and VI, pp. 46 and 49

[2] Wisdom, op. cit. p. 46; cf. A. J. Ayer, *Language, Truth and Logic*, (2nd edn., London, 1955, p. 137—'It "occurs" to him' (the scientist) 'that a certain hypothesis or set of hypotheses may be true'.

It cannot be denied that scientific methods mostly take the form of the hypothetico-deductive method. But a hypothesis arises out of the context of our experience; it is sense-experience that provides the data for the construction of a hypothesis. The only creative power of the mind 'amounts to no more than the faculty of compounding, transposing, augmenting or diminishing the materials afforded us by sense and experience'.[2] But since thought is essentially a process of symbolic transformation and since the mind always follows the principle of least resistance, once the hypothesis is formed we do not care, and hence are not afterwards able, to describe in detail the elements and processes that stimulated the formation of the hypothesis. A scientific theory begins with sense-experience, matures into high level abstraction and ends in a return to sense-experience. But according to Dr Wisdom 'some hypothesis is entertained in the mind before observations are made'. Dr Wisdom reminds us that 'the time factor is important, for it finally disposes of the possibility of induction as a method'. Throughout his 'metascientific' study one observes an insistence on observation, but this observation must be, according to him, a post-deductive observation, and must never be a pre-hypothesis observation. But hypothesis cannot spring into existence out of nothing, it must be inspired by some sense-experience that gives rise to a concrete problem; it must arise out of observation of facts that precedes the formation of a hypothesis.

R. DAS

A REPLY TO DR DAS'S CRITICISMS

DR DAS makes two main criticisms of my treatment of scientific procedure and scientific concepts. I think there may be substance behind his criticisms, but if so this does not come out clearly; the objections he actually makes seem to be wide of the mark.

(i) He begins with my distinction between induction as an inference and induction as a method, and says that nowhere in my work do I explain the distinction. I may not have explained it adequately, but some explanation is certainly offered.

Inductive inference is easy to make clear: it is one in which all the premisses are particular, and the conclusion universal (or else concerned with a new instance of the items included in the premisses). In my work I say of it not that it does not exist but that it does exist and is invalid (p. 223). It is more difficult to describe the method if it is really non-existent. There is no doubt that many scientists have thought they were using an inductive method. If Mill's 'methods' were methods they would be types of inductive method, and they serve to show what sort of thing an inductive method was thought to be. They purported to tell you how to conduct a scientific enquiry: you were first to make a lot of observations, and you could then, for instance, select similarities and/or differences as regards some property and look to see whether other similarities and/or differences also occurred; if you found them, the 'method' gave you a reliable generalisation. I did not describe this sufficiently in my book, though I think it was plain that an inductive method was conceived to be a way of obtaining reliable generalisations from observations (pp. 46, 86).

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[1] Hume, *An Enquiry Concerning Human Understanding*, § II

[2] Ibid.

Do scientists use inductive inferences? In other words do they as a matter of course use invalid inferences? This is answered in the negative by showing that scientific procedure fits the hypothetico-deductive model which does not contain invalid inferences. Do they use an inductive 'method'? Consider a scientist dissecting a whale and finding a heart. If this does not strike him as interesting, nothing develops. If it strikes him as surprising, or even interesting (not quite what he might have expected), he may say, 'I wonder whether all mammals have hearts'. He does not get this by an inductive 'method'; the method, if any, is an imaginative speculative obedience to a craving for generality. There is, nonetheless, an inductive method in a more or less trivial sense: one can specify how to make generalisations, and the recipe is 'Take a number of similar observations, and describe them by the quantifier "some"; then cross out "some" and replace it by "all"'. This, of course, gives a way of obtaining generalisations, but not reliable generalisations; what it gives is a generalisation of a hypothetical kind, which may be tried out tentatively.

These ideas are very simple, but they are at variance with tradition, much more at variance with it than is sometimes recognised. It is often quite difficult to get people to see the point—and I do not mean accept it. So I am not fully persuaded that I explained the matter badly in my book, though I may have.

(i*a*) Das next notes the point that a generalisation should cover the result both of proceeding from particular *individuals* to a universal statement about a *species* and also of proceeding from several particular *species* to a universal statement about a genus. I made this point also in my book (pp. 104 ff.) (though Das believes I omitted it). Here, however, Das has a strong point: he urges that the premisses each of which involves a species, cannot be regarded as being about *instances*. He gives the impression that I could not have included such types of generalisation in my treatment of the subject, because I regard generalisation as something that has instances. In fact I included as instances both individuals and kinds, which is easily understood from my reference to 'determinables' and 'determinates' (pp. 138–9): if several isosceles triangles are drawn, each is an instance of an isosceles triangle; but in their turn isosceles triangles, equilateral triangles, and so on are instances of rectilinear figures. Thus I agree with Das's contention that 'instance' ought to be interpreted liberally, but I have in fact done so. It would have been an advantage to have said so in so many words.

(ii) Das then comes to my thesis that some concepts in scientific hypotheses are 'non-instantial' as I called them. Thus the concept 'gas' has instances, the concept 'gravitational force' has not. (Professor H. H. Price has improved the phrase, and I would now call them 'non-instantiative'.) Das wishes to show that I am mistaken and that my examples are in fact instantiative.

(ii*a*) In discussing Newton's second law, I was concerned with motion produced by a gravitational force (related to a weight suspended over a pulley in an experiment). Here Das makes a mistake : he says that, according to me, the way the law is tested is to repeat the experiment. But what I said was that the law would be tested by deducing the law for a pendulum and finding that this was confirmed by experiment (p. 30).

This mistake does not, however, in itself invalidate Das's criticism, though it may enable us to see an error in that criticism. Testing a hypothesis like the second law (containing, in my view, a non-instantiative concept) necessitates making a deduction of some little complexity. This would not be necessary if the concepts were all instantiative, for the required deduction would then take the form of a very simple syllogism. Thus ' All mammals have a heart ; this object is a mammal ; therefore this object has a heart '. The difference between the types of deduction involved seems to me to show that there is a genuine distinction between instantiative and non-instantiative concepts.

(ii*b*) Das makes a much stronger point when he cites from me another example, that the volume of a gas at constant pressure in proportion to the absolute temperature. He asks, if these concepts are instantiative why not force ? His reason seems to be that pressure in a gas can be expressed as a ratio and so can gravitational force. I do not see the bearing of this. He finds it strange that I should treat pressure as instantiative, but my point may be put like this : pressure is a force that can be felt, and so also is weight, hence they are instantiative ; but gravitational force is much wider, it produces weight, but it cannot be felt as weight can. I can show you instances of triangles, volumes, pressures, weights, but not of gravitational force. I do not think my distinction is free from difficulty, but I do not think Das has found where the difficulty lies. If I were to attack my distinction, I should argue thus. It rests on commonsense knowledge of things around you and on rather simple theories : for instance, when you touch the table, you get an instance of pressure because you have the commonsense knowledge or a simple theory that matter is impenetrable i.e. you ascribe the property of resistance to the table—so why do you not do the same with force and ascribe gravitational force to the stars as a property of matter on a par with resistance ? My answer would be that I can experience the one and not the other ; and, whatever may be the ontological truth about it, the concepts function quite differently in their use. I think, further, that a fundamental difference lies in the fact that instantiative hypotheses always bear to non-instantiative ones the relation of being approximate deductions from these (on this, see J. H. Woodger's admirable analysis, *Biology and Language*, 1953, Part I).

(ii*c*) Presumably with the aim of showing the possibility of regarding all scientific concepts as instantiative, Das cites Whitehead who cited Newton—the law of gravitation, according to Whitehead, ' being purely based upon description of observed facts '. Had I known this passage I could have used it effectively in my book to illustrate the fundamental mistake of the Baconian approach : it is quite impossible to obtain the law of gravitation from the observed facts (it belongs to an altogether different level of abstraction, as Das would say). It is likely, however, that Whitehead meant something quite different by the entire passage, namely that genuine scientific laws, however abstract, have at some point a basic connection with facts.

(iii) I shall end with some brief comments on the rest of Das's discussion.

Das says I do not enquire why abstract concepts in physics are capable of deductive development whereas transcendental speculations in metaphysics are not. The reason I do not enquire is just because this is the way of discriminating the two : a statement being given, one asks whether deductions yielding a possible observation can be made, claiming that if so it is scientific, if not metaphysical. Das, on the other hand, says that transcendental concepts are incompatible combinations of basic concepts. This surely means they are self-contradictory ? If that were so, it would be an easy matter to refute metaphysical systems.

Misunderstandings, mistakes, faulty exposition, and so on apart, what is behind the issues Das has made ? Through what he says there runs a certain thread. He contends there are different levels of thought and abstraction (naturally I agree) ; he does not accept Vaihinger's view that all abstract entities are fictions (nor do I, though Das thinks I do) ; he does not believe that what is ultimately real includes only individual things (nor do I, though Das thinks this is one reason why I hold that force is non-instantiative). It is clear that his theme is reality. My theme on the other hand was the nature of scientific theory or explanation. I do not hold that abstract high-level explanatory concepts are necessarily fictional, but some undoubtedly are. Though we now know that atoms exist, there was great doubt about this at the time when Dalton's theory was developed ; yet it would have been just as effective an explanatory theory even if atoms did not exist. Again force of attraction is a fiction, as may be seen from the fact that the concept is not required in general relativity. Das believes that if such concepts were fictional no useful purpose could be served by them and nothing observational deduced ; the history of science surely shows otherwise. But the history of science does not show that all such concepts are fictional. Hence my treatment of the nature of scientific theory did not really carry any implications about the nature of reality. It seems, however, that Das has opposed it and to some extent misunderstood it at least partly because he thought my treatment excluded from reality what he would include.

In raising this theme, Das is at the same time raising a baffling metascientific question : so far as some explanatory concepts are either non-instantiative or fictional, how is it that they can play their explanatory part ?

In conclusion I may mention that Das appears to credit me with some ideas that in fact are due to Popper. The basic general theory, including the view that all induction is a ' myth ', is all Popper's. Das is right in ascribing to me the distinction between induction as a method and induction as an inference, and between instantiative and non-instantiative concepts, which I used, before I knew Popper's work, to show that, whatever else might be inductive, non-instantiative hypotheses could not be obtained by induction. But Popper introduced the general theory that scientific hypotheses are discriminated by refutability and that observations function not as a base upon which to build hypotheses but as a basis for testing them.

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REVIEWS

THE SCANDAL OF PHILOSOPHY

Mr Roy Harrod has written a vigorous and instructive book [1] which does something to bring to a focus—and to a head—the two hundred years of argument about induction. He is a partisan in the argument; his book is not meant to be neutral and judicial; but he does marshal, in planning his own approach, what other writers on induction have said and may have meant. Formed and informed by Mr Harrod's lively mind, this exposition is better than any textbook history. *Foundations of Inductive Logic* will be used by philosophers of all schools as a source of pithy quotations, which can be counted on to challenge the dullest student and strike an essay from him.

This description will suggest that Mr Harrod's is, in some ways, an unsophisticated book: the blacks are very black and the whites are very white. He is not at all browbeaten by Hume's bland and explosive questions; his purpose is to set up induction again as it stood before 1739, as a principle in its own right—lock, stock, and barrel. Mr Harrod is fair to what has been written since then, but he cannot help at times showing a certain impatience with it, as if he thought the sceptics perverse or even insincere. Arguments against induction have a family resemblance to arguments for solipsism; and Mr Harrod's manner here and there calls to mind Dr Johnson outside Harwich church in 1763, when he was challenged to refute solipsism. Dr Johnson on that occasion kicked a stone, but it is plain that he would have preferred to kick the sceptic.

I

Mr Harrod's aim is ambitious. He is not content, for example, to propose induction merely as a working policy. He claims that his reasoning in this book gives an absolute justification for induction: nothing less.

Mr Harrod believes that his justification is not based ultimately on our experience of nature. In a striking passage, he writes:

> The principles of induction have equal logical status with those of deduction.
>
> If our universe did not have those characteristics, which happily it manifests, of continuity, repetition and observables complying with simple laws, the principles of induction would not thereby be rendered invalid; but there would be lack of scope for their application (p. 261).

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[1] *Foundations of Inductive Logic.* By R. F. Harrod. Macmillan, London, 1956. Pp. xviii + 290. 24s.

Accordingly, Mr Harrod has no truck with those large postulates on which other writers have tried to base a justification of induction : such postulates as the uniformity of nature, say, or the existence of natural kinds. His method is to begin from first principles, and he insists that in his book he has made no assumptions about nature.

> The greatest care has been taken, in justifying induction, to make no presupposition about the character of the universe. This rule will be adhered to rigidly throughout (p. 120).

Thus Mr Harrod sets out on his task with, apparently, a minimum of equipment. He conceives the task to be, to find a principle on which he can base a forward-looking decision at each moment of experience. Mr Harrod pictures experience as a journey, and we are taken aback to find that the journey is assumed implicitly to lie in a continuous medium.

> Before having made any inductions, man could know nothing whatever, except what is under his nose. Consider a journey by such a nescient man along a continuity. The continuity may consist of a uniform colour, texture or sound, or of a repeated pattern, where the pattern is easily cognizable at a glance, this uniformity being in contrast with a heterogeneous background. The set of premises of the class of arguments in probability that he can develop are constituted by the facts that successively arise as he proceeds. The common quality A is the fact of travel on *this* particular continuity, e.g. this particular strip of uniform colour (p. 52).

To this nescient traveller, Mr Harrod proposes a defined procedure. He asks him to choose, before he begins his journey, some fixed fraction—say one-tenth. Then the traveller is to act on the belief, at each point in his journey, that the continuity which he has met so far ' will continue for a length constituting at least one-tenth of the length for which it has already proceeded ' (p. 53). In other words, going out as it were on to the glacier of experience, the traveller is to back his judgment in advance that at each point he can risk a step one-tenth as long as the distance he has already come without stepping into a crevasse.

This is an attractive formulation of a way in which we often have to act. The procedure Mr Harrod recommends is sensible, in that it is often practical and usually unavoidable. And it has the merit, as he points out, that

> it is quite certain that we shall be right ten times for every once that we are wrong. We shall be right during the first ten-elevenths of the journey and wrong during the last eleventh (p. 53).

Of course, this may be cold comfort at the last moment, when we pitch into the crevasse ; but, in the nature of things, that is an accident against which nothing can insure us. At the same time, Mr Harrod's procedure is certainly not universal. Consider, for example, a man's progress through his own life. Mr Harrod invites him to plan at twenty on the assumption that he will live for another two years. But if the man survives to eighty, Mr Harrod asks him to plan the rest of his life on the assumption that he will live for another eight years. Even the nescient man would sense that this policy, however human, is not reasonable.

2

In spite of Mr Harrod's disclaimers, his method is plainly tied to the nature of things. And it is tied specifically to the nature of everyday lifeless things. The world in which alone it is valid (and meaningful) is the physical world we know from day to day.

For example, Mr Harrod begins by making his imagery, and ends by making his reasoning, hinge on the properties of space as we know it. He pictures the progress of his nescient man as a journey; and if this picture is to be universal, then the journey must be not in space but in something more general and abstract—we can call it an experience-space. Mr Harrod silently assumes that this abstract experience-space has the same properties as the Euclidean space in which we move physically. There is no warrant at all for this, and it makes his argument merely a crude analogy. The experience-space may have a metric which quite distorts his succession of fractional steps. Or it may be fissured everywhere by discontinuities. It may be impossible in it to take the first step which Mr Harrod needs without at once meeting a discontinuity.

Let me put this in another way. Mr Harrod summarises his argument, as he says, ' in popular words ' as follows : ' If we are crossing an expanse, but know not what part of it we have reached, we are unlikely to be on its extreme edge ' (p. 78). But this supposes that the expanse is of the kind to which we are used, an island or a table-top. It is however possible to construct an expanse which is not like this, and whose edge meanders close to every point on it. And we cannot know that experience will not be an expanse of this kind.

All this makes it very clear that Mr Harrod does not justify induction. What he does is to propose a particular policy on which we are to make inductions : and if we follow this policy, he promises us a reasonable proportion of successes. He bases this promise on a postulate which has some kinship with the postulate that nature is uniform. Mr Harrod's postulate is that nature is continuous—more precisely, in the language of mathematics, that nature is continuous ' almost everywhere '.

There is of course no objection to Mr Harrod's giving, in place of the promised justification, only a policy of induction. For it has been evident for a long time that nothing more is possible. There is no way to argue the sceptic out of solipsism and, similarly, there is no way to argue him into induction. Scepticism about the behaviour of things has much the same scope as scepticism about their existence, and the one can be rebutted only as Dr Johnson rebutted the other, by showing that it has no possible bearing on our actions.

There then remains the question, whether the policy of induction which Mr Harrod recommends is a good one. This is surely very doubtful. Mr Harrod's policy is founded on a postulate of continuity, and this postulate is adequate for everyday experience. But the postulate has long been inappropriate to the most interesting field of new experience open to us—the field of science.

It is ironic that when John Stuart Mill in 1843 proposed the postulate that nature is uniform, it had already been breached by the discovery in 1828 of the Brownian movement. But Mill at any rate died before the discrete nature of the Brownian movement was fully understood, and its discontinuous origin explained. Mr Harrod lives in a world in which radio-active decay, the behaviour of liquid helium, and a thousand other quantum phenomena are commonplaces. In choosing a policy of induction which avoids these pitfalls, he is closing his work to the very parts of experience in which induction is still an adventure—and a practical adventure.

3

It is proper to consider Mr Harrod's robust but limited book in the wider setting of scientific method. Here after all is where inductive generalisations have had their greatest success, and where new generalisations are still made. In what way are they made? How does science set about formulating the great nexus of its laws which Mr Harrod ignores? What are the units of experience from which it begins, and of what kind are the connections between them which it proposes?

Logicians commonly take the view that these units are observations that 'an *A* is a *B*', and that from the accummulation of such observations we acquire the habit of expecting that 'all (or many) *A*'s are *B*'s'. Professor Donald Williams, for example, used the statistical theory of sampling to show that from observations of this type we have some right to infer an expected distribution of similar observations in the universe. And Mr Harrod, in an interesting section of his book, shows that his own method can be used to cover and strengthen Professor Williams' inferences about observations of this type.

But observations that 'an *A* is a *B*' are made only in natural history; such observations, and the accumulation of them, are foreign to the higher levels of science; and scientific statements are put into this form only by an artifice. The natural form of a scientific statement says that if we do *a*, then *b* happens. The practice of science is active, and is concerned not with the appearance of *A*'s but with their behaviour. In science, we have to find and indeed create for ourselves the likenesses which we then describe in inductive laws; and these spring to our minds, not as instances, but as relations.

There are no natural kinds in science ; there are only constructed kinds, and they are kinds only because they obey laws.

Non-scientists usually conceive induction as Mr Harrod does, as a method to forecast single facts—the next step, or another instance. But an induction is not a forecast : it is a proposed law. An inductive procedure yields not an instance, but a generalisation, and its instances can be derived only from the generalisation. Every induction is from the beginning the formulation of a law.

These laws differ from those which are derived from prior laws or axioms by deductive steps. But they do not differ because deduction is in some way sounder than induction. If we apply deduction to the physical world, it becomes subject to the same test of experience as induction does. Hume's famous taunt, that there is nothing necessary about the behaviour of two billiard-balls, applies equally to the superposition of two Euclidean triangles.

The reason for our confidence in deductive procedures is, very simply, that they give unique answers. The reason for our doubts about inductive procedures is that they do not. The forms of inductive reasoning do not lead to unique laws : they are many-valued.

Because it is possible to construct many alternative inductions to cover the same set of facts, it is necessary to weigh one of these inductions against another. When reasons are given for preferring one induction to another, it is often said that the preferred induction is more probable. This is in several ways an unhappy use of the word ' probable ', even among philosophers who take care to distinguish it from that more objective use which describes a specified distribution of events. Mr Harrod discusses these probabilities, but it was not to be expected that he should find anything new in this difficult and debatable field.

4

In science, the choice between alternative laws is directed by the weight which the evidence offers for each. These weights do not exist in themselves ; they have a place only in the context of other laws which are already established. Thus the choice of a new law is made in the context of known laws, which the new law fills out and in turn modifies.

In such a system, induction is not a simple progress—a journey step by step by a nescient man. Induction in science is the fitting of one law into a nexus of laws, and the test for it is the fit. The new law must slip into place snugly, but it must do more than this : it must link and lock the existing laws, and tie them together so that they are seen to form a closer unity. The new law must improve the organisation of the older laws, so that their totality now carries more information or meaning.

This is the problem of induction that interests the scientist. He wants a measure of the weight of evidence, and we see that this means that he wants a measure of the organisation within a system of laws. This is almost a topological problem : a problem of enumerating a set of linkages and combining them into a single measure. I have proposed elsewhere [1] one possible solution, and there are others ; indeed Ockam's razor is, in essence, a rudimentary solution. This is not the occasion to discuss such solutions. But it is the occasion to state again the scientist's problem, and to recall how much larger and more interesting it is than the narrow inductions which Mr Harrod has explored.

J. Bronowski

METHODOLOGY AND QUANTUM PHYSICS

The Proceedings [2] of the Second International Congress for the Philosophy of Science, which was held in Zürich in August 1954, contain 101 articles in five volumes. They reflect to a remarkable degree the main problems which beset the philosopher of science in our times.

While the attempts to solve these problems vary considerably between the different countries and various schools of thought—sometimes to the extent of contradicting each other—the major philosophical problems which trouble us today are largely the same throughout the Western world. Any attempt, however, to identify a particular country with one particular school, or with the preoccupation of one special problem, would have to fail in view of the diversity of opinions and methods discussed and exhibited in any of them—with the exception of the Russian school.

The problems that are dealt with, and the proposed methods of their solution, cut across the borders between the nations.

In none of the other main participating countries, viz. Germany (16) [3], Switzerland (11), France (11), U.S.A. (10), and Italy (10), did any particular school preponderate , though an empiricist tendency was noticeable everywhere. There was, perhaps, an inclination toward Rationalism and Neo-Kantianism in some of the German papers and a prejudice in favour of

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[1] J. Bronowski, 'The Logic of Experiment', *The Advancement of Science*, 1952, 9, 289-96

[2] Proceedings of the Second International Congress of the International Union for the Philosophy of Science, Zürich 1954. Editions du Griffon, Neuchâtel, Switzerland, 1955.

[3] The numbers in brackets indicate the number of papers presented by each country.

Conventionalism in some of the French, but both these trends were represented in other countries as well. The most striking feature in this respect was the absence of representatives of Continental Existentialism (with one or two exceptions), which might be explained by the fact that Philosophy of Science does not lend itself readily to ontological considerations.

There was, however, a considerable Realist undertone in many of the papers of all countries which became even more apparent in the discussions. These were characterised by definite climates of opinion, sometimes strongly divergent and even hostile to one another. For instance, during one of the discussions of papers on Physics, the Chairman said that there could be no doubt that Logical Positivism had run its course and could now be considered dead and buried—and no-one in the audience of thirty or forty people rose to contradict him. On reading the Proceedings of the Congress now, however, more than two years later, one finds that this extreme opinion, which may have been characteristic of a certain audience, is not reflected in more than half a dozen papers in all.

The hardest to classify are clearly the French and the Swiss philosophers. While their views are generally those of Empiricists, few of them employ the method of logical (let alone linguistic) analysis, and their approach to Epistemology is quite unique and unorthodox. Some of the most interesting and original papers—which I shall mention in some detail later on —are by Swiss or French authors.

In Italy, too, the empiricist attitude prevails. There was no scholastic or theological, and only one ontological, paper ; also a brilliantly written critical essay (' L'Intention réaliste dans la physique moderne ') on the positivist interpretation of modern physics by Professor Selvaggi, a Jesuit with strong leanings toward Realism.

The American articles dealt mainly with Logic and Physics, while the English papers were all grouped together under Psychology, except Dr Hutten's paper on the Pauli Exclusion Principle which aroused an interesting discussion in the Physics section and was excellently defended by its author, and a lecture by Mr L. L. Whyte on the ' Heuristic Value of the Philosophy of Physics ' which, like a number of other papers, did not appear in the Proceedings.

The Congress met in morning and afternoon sessions for five days, the plenary morning sessions being dedicated to General Expositions of about an hour's length, while the afternoons were given to simultaneous discussion groups on short (15 minutes) papers in the following sub-divisions: Physics, Mathematics, Theory of Knowledge, Linguistics, the relation between Science and Philosophy, History of Science, Sociology, and Psychology. The Proceedings were classified in the same way, but with several papers the classification must have been an arbitrary one. Logic was not treated as a separate branch, but one of the afternoon Symposia which continued throughout the week under the Chairmanship of Professor Bernays dealt with the Foundations of Logic and Mathematics. There was, in addition, one paper given in the Plenary Session by Arthur Pap on ' Extensional Logic and the Laws of Nature '. Three of the seven published mathematical papers concern the Intuitionist programme of reconstructing parts of mathematics.

One of the main problems dealt with at the conference was that of Quantum Physics, and it focused on the Heisenberg Uncertainty. It was felt (as Professor Destouches (Paris) put it in his article ' Sur le débat actuel concernant la connaissance physique ') that *the philosopher today finds the theory of knowledge suspended in a debate over pure physics*—a position which he may consider quite unacceptable. The proffered solutions of the problem were varied and, as it seems to me, made without much conviction. Complementarity was almost generally rejected as being unhelpful. Among those who accept the statistical interpretation, opinions were about evenly divided : some of them maintained that a three-valued logic is not only necessary for a *description* of quantum-mechanical situations, but actually *demonstrated* by the natural phenomenon itself ; while the others believed that the uncertainty concerns only the *predictability* of the particle's behaviour, i.e. our own state of knowledge or ignorance. And, finally, there were those who think that if we could only find the ' hidden parameters ', the microcosmic world would reveal itself to be as causally determined and determinable as the world of classical physics.

Professor Destouches made the additional two important points, that (*a*) a complete revision of basic concepts (such as : ' corpuscle ', ' measurement ', ' observer ', ' prediction ', etc.) is necessary if we want to give a consistent and exhaustive description of quantum-mechanics that is also transformable into one of classical physics ; and (*b*) that the rôle of the measuring apparatus, being part of the observer, cannot be neglected in quantum theory as it can be in Newtonian theory.

A few papers dealt with Relativity and its relation to quantum-mechanics, and several others were devoted to a more general epistemological problem arising from modern physics, namely that of Determinism and Causality. The distinctions between various types of determinism were discussed (viz. mechanical, materialistic, physical and ontological), and it was maintained by some that one could uphold ontological determinism while rejecting all the others. A French author, J. Moreau, charged the Germans with using ' causality ' and ' determinism ' synonymously and contended that determinism need not contain the causal factor, but merely permits prediction, to the degree of certainty, on the basis of past data.

The most devastating impression created by the various representations of this fundamental problem in modern physics, and in modern epistemology, is that on all sides assertions are made, not merely concerning the status of our knowledge, but about nature, ' the real world ', itself. Instead of discussing the respective logic as a convenient, or simple, or in some other way preferable *description* of an observed or inferred phenomenon, we get statements that, ' clearly ', *natura non salta facit*, or that the world behind the phenomena is, ' clearly ', indeterministic. It would be unfair to many of the Congressionalists to say that this was a universal attitude—but that *anyone* should wish to commit himself so far is, in view of the present state of theoretical physics, highly surprising and regrettable.

While the authors on physics merely presented their own views and were often quite unyielding in the discussion, the speakers in the Plenary Sessions seriously tried, not only to examine, but also to bridge over, the more common epistemological problems of our day. Following, I take it, the professed intention of the Congress ' to confront and to reunite the different trends in the Philosophy of Science today ', most of them dealt with it as a discipline and contrasted two or more of the prominent schools of thought, with the aim of dissolving much of the contrast.

The President of the Congress, Professor Gonseth (Zürich), developed his contention that philosophy is a ' discursive activity ' and that both science and philosophy must remain ' open disciplines ' in the same way in which language is ' open ', i.e. revisable and corrigible. Knowledge proceeds by trial and error, he said, and the greatest danger confronting us today lies in the attempts to present philosophy of science as a fixed, static subject, incapable of improvement and adjustment to the continuous advances of science itself.

While Gonseth believes that language is neither prior nor consequent to knowledge, but a concomitant form of it, Professor Piaget (Geneva and Paris) wants to show that the roots of logical thinking go back into the prelinguistic stages of human development. In his ' Epistémologie génétique ' he formulated the results of his experiments with small children who, he claims, establish the meaning of concepts operationally, i.e. by physical activity which includes handling, weighing, comparing, and counting the objects, and thus forming the ideas of their weight, size, number and, finally, their identity and relation to one another (such as : ' larger than ', ' lighter than ', ' the same as '). The abstraction of the *concept* of identity, negation, reciprocity, etc., is genetically a much later one. Genetic order reverses logical order, he says, and it is therefore false to base logic on language. A mental activity which we call ' thinking ' characterises the small child long before any norms of symbolic expression influence his conceptualisation. One might wonder whether Piaget is justified in equating sensori-motoric processes, to which he refers in the case of the infant, with actual reasoning. Nevertheless, his ingenious attempt to dissolve the dualism between structure and genesis of logical concepts represents one of the most original and systematic studies in continental epistemology today.

In his paper on 'The Philosophy of Science of Logical Empiricism', Professor Feigl (Minnesota) deals with 'a few fundamental points in the epistemology and methodology of the empirical sciences'. The two problems which he singles out are : (*a*) the disputed division of all statements into analytic and synthetic—which he maintains as being helpful *as long as* one extends their use in such a way that the same proposition can be either analytic (as a definition), or synthetic (as a law), depending on its use ; and (*b*) the Empiricist criterion of meaning which he wants to see applied in a liberal and wide sense, so as to include inductive inferences. He believes that Logical Empiricism has recently developed a more Realistic trend which distinguishes it from the strict Positivism of the Vienna Circle up to 1930, and which is equally far removed from Operationalist and Phenomenologist schools.

His optimistic prognosis of Empiricism was sharply rejected by Professor V. Kraft (Vienna) in a paper called 'Der Wissenschaftscharakter der Erkenntnislehre', where he said that one should not treat knowledge ('Erkenntnis') as something that existed as 'given' and that we only had to *discover*, but that it had to be *defined*. To him, knowledge is not a reality which can be ascertained, but an aim which we wish to achieve. The various theories of knowledge (Intuitionist, Conventionalist, Empiricist, etc.) can be distinguished by their criteria of truth and verifiability and their method of acquiring knowledge (Erkenntnis). But such a relativism of the concept of knowledge is useless, since we need one, and only one, if philosophical discussions are to be fruitful. We must therefore define it and incorporate it into an axiomatic system of definitions and inferences (a hypothetico-deductive system). Only then will theory of knowledge become a *theory* in the true sense of the word.

A somewhat intermediate position is advocated in two other papers, one by P. Filiasi-Carcano (Rome) who contrasts Positivism with the 'non-positivist schools' and who has several kind things to say about both, e.g. that the Existentialists are right in emphasising the rôle of the human mind as a medium for the acquisition of knowledge and an instrument for the process of knowing. He thinks that the central problem in the Philosophy of Science is to ascertain the reality 'behind' the phenomena, and that in this sense all scientists could be called 'Realists'.

The second of these papers ('Prolegomena einer kritischen Philosophie') is by Professor Reidemeister (Marburg) who advocates what he calls 'analysing thinking' ('analysierendes Denken') which he considers to be an irreducible (I suppose he means innate) ability of the human mind to distinguish between sense and nonsense. Both Positivism and Existentialism ignore this postulate of understanding and use their non-understanding ('Nicht-Verstehen-Könenn') as arguments in their cause. They represent the extreme poles of modern philosophy, but resemble each other in this respect. He makes a plea for the return to a philosophy based on *reason*, as classical philosophy was.

Two further papers deal with method in the Philosophy of Science: Professor Perelman (Brussels) discusses ‘Le Rôle de la décision dans la théorie de la connaissance’. He cites two extreme cases, that of logic, where decision plays no part at all, and that of law, where it is paramount. He says then, that the scientist, when confronted with a new experimental result, merely has to decide whether to incorporate it into an existing system, or to invent a new theory, and that his choice will be guided by such notions as simplicity, regularity, and economy of thought. The methods of science are as varied as the sciences themselves, and it would be misunderstanding the idea of the Unity of Science to believe that the same method could be applied everywhere.

G. Klubertanz, a member of the Society of Jesuits, and of St. Louis University (U.S.A.), believes, on the other hand, that there exists a latent unity behind the divers standpoints in the Philosophy of Science, and suggests a ‘Program for Progress in the Philosophy of Science’ which would help to establish such a unity of method. He mentions a number of types of data which ought to be considered, namely: theories of generalisation, of concept-formation, of deduction, of verification, and of formation of hypotheses. By concentrating its efforts on these topics, Philosophy of Science could be made into a systematic and uniform discipline.

Finally, there is a paper by B. M. Kedroff (Moscow) ‘On the Classification of the Sciences’ where he says that the sciences fall naturally into a linear order, viz. mathematics—mechanics—physics—chemistry—biology—sociology, with the new sciences (e.g. bio-chemistry, micro-mechanics) having developed on the border-lines between them. He believes that this order is not arbitrary as, according to him, the Positivists maintain, but objective, that is, given by nature itself.

Some of the shorter papers presented in the afternoon sessions also dealt with the more general aspects of method. A rather obscure paper by Professor Hugo Dingler (Munich) discussed the Operationalist method, using an example from technology, namely the construction of artefacts (e.g. measuring apparatus) whose exactness in measurement is not arbitrary, but *defined* by the construction, so that they enable predictions to be made which approach the probability value 1 (certainty). These artefacts thus make it possible for us to determine the nature of reality (i.e. of a certain matter of fact) *to the limits of observability*, and this alone allows us to assert the existence of laws of nature (surely a rather absurd conclusion to draw).

A much more convincing argument was presented by a compatriot of Professor Dingler, Gerhard Frey (Stuttgart) who maintains that a purely operationalistic definition of knowledge excludes all self-reflection and self-knowledge from the (verifiable) domain of philosophy. The process of *understanding* can therefore not be represented operationally, nor can any kind of infinite process, since it is not constructable. Of the two parts of scientific research, viz. the *discovery* and the *verification* of theories, only the latter can be demonstrated by operational methods. Science as a whole can therefore not be described by them.

Several papers were concerned with the formation of hypotheses, a problem which, I believe, is continually increasing in importance today ; some others dealt with the influence of science on philosophy (which was asserted as being positive), and vice versa (this was believed to be sometimes detrimental to science) ; and still others examined the difference between the *structures* of the natural and the social sciences.

There were only about half a dozen articles on Linguistics, but these were interesting. P. Belov (Moscow) went so far as to say that *language gives birth to our ideas* and that the unity of language and thought has been well demonstrated by modern researches on cerebral activity. The word presents itself as an instrument of expression to the thought and penetrates into consciousness. He believes that the formalisation of languages into symbols is a hindrance, rather than a help, to their understanding.

An essay by Rudolf Freundlich (Graz) who discusses the interpretation of a linguistic calculus, modifies this extreme view by stating that the final meaning-criterion of a symbolic language must always be sought in its translation and translatability into a natural language. H. Glinz (Switzerland) goes even further and sees a detrimental influence in the fact that our own language imposes certain thinking habits upon us, from which only a universally accepted formalism could free us.

Of the eight articles in the field of Sociology, two deal with the structure of the social sciences. E. Walter (Zürich) proposes a division into theoretical and experimental sociology, analogous to that in physics. And W. R. Dennes (Berkeley) discusses its similarity to the natural sciences as far as the collection of data and the establishment of hypotheses is concerned. A rather interesting Russian paper by F. V. Konstantinov denies the existence of the individual outside of society, to which he owes not only all his qualities and characteristics, but also all his ideas and views. ' There are no Robinson Crusoes in society ', he says. (Luckily for the Robinsons among us, this is true only for some societies !) A note by J. Mokre (Graz) compares two types of law, viz. ' positive ' law and ' natural ' law, and states the now generally accepted thesis that positive law, which is a practised, legalised formal system, should approach as closely as possible to natural law, i.e. the moral ' intuition ' which has developed in man, possibly as a matter of social necessity.

The English articles on Psychology are all critical discussions, excepting perhaps J. O. Wisdom's paper ' Is Epiphenomenalism Refutable ? ', where he convincingly argues that it is not, and cannot therefore be called a theory at all. C. K. Grant (Nottingham) deals with Ryle's denial of the logical possibility of explaining mental phenomena by causal theories—as it is attempted in Psychotherapy—but I find that Mr Grant was entirely too kind to Ryle's argument. And, finally, W. Mays (Manchester) gives an appraisal of Professor Piaget's ' Epistémologie Génétique '.

None of the psychological papers, then, deal with Psychology as a science—its postulates and assumptions, its scientific and humanistic elements —nor with the claims made by the various schools of thought. Could it be that Psychology is not yet accepted as an equivalent science among other sciences ? And yet, is it not plausible to expect that an interaction between philosophy and psychology might become one of the most fruitful enterprises in the Philosophy of Science and for the development of a scientific theory of knowledge ?

These volumes represent a sublimate of the main thoughts in the field of scientific philosophy that exist today. Its contents are stimulating and reassuring. They emphasise the need to become aware of what others are doing in the field of philosophy of science, so that we may pool our efforts rather than duplicate them.

Eva Cassirer

The Idiom of Contemporary Thought. By Crawford Knox.
Chapman and Hall, London, 1956. Pp. 206. 18s.

When an accepted authority in one or another field of thought transcends his special subject to seek more comprehensive relations, his former followers are apt to halt uneasily, to murmur ' He was all right until he went mystical ', perhaps to feel vaguely resentful. The amateur or dilettante who seeks similarly is more comfortably dismissed : he has, it may be suggested, no discipline at all.

It is probably true that the urge to generalise concerning all fields of knowledge and belief is responsible for more bad books than appear in the name of any one special discipline, but this only indicates the need to establish standards ; the urge is to be encouraged, while making it clear that the task is more difficult, not easier, than that of specialisation.

This book presents a careful attempt to convey, briefly, the conclusions of one man's exploration. It consists of two essays, in the first of which the author seeks something common to the subject matters of the physical, biological, and psychological sciences and of aesthetic and ethical philosophy, and to mystical experience. To avoid unwanted connotations he names this ' the medium '. It is the ultimate ' that which behaves '. Its behaviour (experienced via the senses) is the subject matter of the physical sciences, i.e. the properties of its behaviour are physical. Its own properties (directly experienced by minds) include life and consciousness, including self-consciousness and the class of experiences sometimes referred to as 'mystical'. This concept is first presented as part of a model resolving the body-mind difficulty. Its relevance to 'understanding' such aspects of physics as the apparent discontinuities of existence or causality in quantum mechanics is suggested (the 'medium' exists continuously; its behaviour is intermittent). Finally, evidence of psychical phenomena and religious experience is interpreted in terms of this same 'medium'.

The second essay considers personal, social and religious ethics, using some of the concepts of psycho-analysis, with infrequent glances back to the system of metaphysics offered in the first essay. Three interacting aspects of personality—adaptation of the individual to society, internal adjustment, or stability of the individual, and integration—are presented as touchstones for value judgments on types of society.

Rather than a contribution to literature on the philosophy of science, the book may perhaps be regarded as a consequence of it. Alongside attempts to convey eastern and western modes of thought and experience the author has ranged the attempts of some scientists to convey *their* modes of thought, and has searched for a common referent. The word 'medium' is unfortunate, with its unavoidable sense of intermediacy which it shares with 'Tao'. The concept is very close to those expressed by 'essence', 'substance' (a ruined word) and, more particularly by, 'Brahman' and Whitehead's 'creativity' (not, I think, by Whitehead's 'God'). Perhaps Mr Knox's choice of word is a concession to the fashionable attitude to metaphysics, on which a comment here may be appropriate.

The truism, that a metaphysical proposition cannot be verified, is commonly held to entail that metaphysics is a waste of time for the scientist, irrelevant to his work. This is a superficial judgment. One is tempted to say that illumination must precede reflection. States of mind (such as being in love, being happy, being miserable, to mention some that are perhaps more familiar than those of mystical exaltation) do influence the emergence of novelty in scientific thought. Yoga might have an obstetric influence on the emergence of an inductive generalisation; the fact that it provides no support for the validity of the generalisation is neither here nor there.

The 'medium' is seldom referred to explicitly in the discussion of moral philosophy; implicitly, it appears in references to 'God'. I found this second essay interesting rather than stimulating, with echoes of the hygienic, rather cool, joyless recipes I associate particularly with some post-war writings of German ethical philosophers; something vital seems inevitably missing from their Utopias, however carefully propounded—none, I think, do there embrace.

G. E. Denyer

Mathematical Foundations of Quantum Mechanics. By John von Neumann,
translated from the German edition by R. T. Beyer.
Princeton University Press, 1955. Pp. 472. $6.00

Elementary quantum mechanics as presented in the early thirties was a highly successful theory, but from the point of view of rigour it suffered from various defects, mainly two : it used mathematical procedures that were not permissible, and even inconsistent ; and its interpretation, viz. the Copenhagen-interpretation, although a useful guide for the theoretician, a good means of orientation for the experimentalist as well as an exciting subject for popularisation it was far from clear what exactly its presuppositions were.

The treatise at present under review, which appeared first in 1930, can be regarded as an attempt at a presentation of the theory (elementary quantum mechanics without spin) which is free from those two defects. The first defect is removed by developing an extension of the theory of the Hilbert-space and by a corresponding reformulation of the eigenvalue-problem. We shall not concern ourselves with this part of the treatise (chaps. I, II), which has found little appreciation among physicists as it ' involves a technique at once too delicate and too cumbersome for the . . . average physicist ' (E. C. Kemble). The second defect is partly removed by an attempt to base the whole theory upon the formalism together with ' a few general qualitative assumptions ' (p. 295). Although this attempt has not been completely successful, it is still of importance as an attempt (*a*) to discover just what exactly is implied by quantum mechanics and what must be left to speculation and (*b*) what are the assumptions which must be added to the formalism in order to turn it into a fully-fledged physical theory.

It has sometimes been thought that those assumptions are provided by the Copenhagen-interpretation. This view is incorrect as the Copenhagen-interpretation is neither necessary nor sufficient for connecting the formalism ' with reality ', i.e. with the results of measurements. It is also fairly independent of the formalism, as it consists mainly in interpreting the quantum-postulate and in drawing conclusions from it which may be said to be elements of a new (and non-mechanistic) ontology. This at once raises the question to what extent this ontology is justified, not by speculation on the basis of the quantum of action, but by the theory as a whole ; or, to be more precise, it raises the question which of those assertions are implied by the theory and which are based upon (ontological) speculation only.

Von Neumann's treatise attempts to provide an answer to this question. I shall use the rest of this review for a critical outline of this answer.

Interpretation : The rules that are used for connecting the formalism with 'reality' are: (1) two rules (I and II on pp. 313 sq.) connecting physical magnitudes with operators of the formalism; and (2) the Born-interpretation (p. 210).

Rules (1) are not the only rules which are possible. Weaker rules may be used (and have in fact been used e.g. by Weyl) which do not any more enable us to prove the theorems referred to below under (*a*) and (*b*). There seem to be strong plausibility-reasons for adopting von Neumann's rules. Yet the existence of weaker alternatives shows that the so-called 'Neumann-proof', i.e. the attempt to prove that (A), (B), (C) below are not independent assumptions but follow from QM, is by no means as strict and straightforward as it may appear at first sight. Rules (2) are deduced from rules (1) and some assumptions about the properties of expectation-values.

Probability is introduced in two steps; firstly, in the usual way, viz. as the limit of relative frequencies within large *ensembles* (von Neumann adopts the Mises-interpretation of. probability (pp. 289 sq.)). This procedure, which has greatly clarified the rôle of probability within quantum mechanics, is suggested (*i*) by the fact that the Born-interpretation is a statistical interpretation in a straightforward and classical sense and (*ii*) by the fact that the statistical properties of large *ensembles* can be studied by experiments upon small samples, whatever happens during those experiments (p. 301). The statistical properties of the *ensembles* are then completely and unambiguously characterised by their so-called statistical operators. In the second step, which is usually overlooked by those who interpret quantum mechanics as a variety of classical statistical mechanics, it is proved (*a*) that every *ensemble* of quantum mechanical systems is either a pure *ensemble*, or a mixture of pure *ensembles* and (*b*) that the pure *ensembles* (1) do not contain *sub-ensembles* with statistical properties different from their own (they are 'irreducible'), (2) are not dispersion-free. This proof (the famous *Neumann-Proof*), which, apart from a certain set of correspondence-rules (see last paragraph), also presupposes the (hidden) assumption (we may call it the *completeness-assumption*) that the statistical properties of any *ensemble* of quantum mechanical systems can be represented by a non-negative hermitian operator (or, in other words the assumption that any *ensemble* of quantum mechanical systems is either a pure *ensemble* or a mixture of pure *ensembles*), allows for the application, *to individual systems*, of probabilities in the sense of relative frequencies; furthermore it is supposed to show that the assumption of universal determinism (even of a 'hidden' determinism which underlies the apparently statistical behaviour of microscopic systems) contradicts elementary quantum mechanics in its present form and (in the above) interpretation that two important elements of the Copenhagen-interpretation, viz. (A) *indeterminism* and (B) the *interpretation of Heisenberg's relations* as restrictions of the applicability of the terms involved, do in fact follow from the theory; and that it is impossible to separate the individual system from the *ensemble* to which it belongs (if this *ensemble* is a pure case);

this corresponds (C) to Bohr's idea of the *indivisibility of the phenomena.* But as the *completeness-assumption* is one of the hidden presuppositions of the proof all that can be said is that the Copenhagen-interpretation is consistent with QM rather than it follows that from QM interpreted in accordance with the above-mentioned rules (for this point cf. Groenewold, *Physica* 12; a similar point was made by Popper in 1935).

Here I ought to make two remarks; firstly, even if (*a*) and (*b*) were theorems of QM von Neumann's proof could not show, as has sometimes been assumed, that determinism has been eliminated once and forever. For new theories of atomic phenomena will have to be more general; they will contain the present theory as an approximation; which means that, strictly speaking, they will contradict the present theory. Hence, they need no longer allow for the derivation of von Neumann's theorem. This does not mean, however, that they will perhaps contain 'hidden' variables (who would possibly defend the view that one day, perhaps, we might return to phlogiston ?). Secondly, none of the attempts to refute that part of von Neumann's argument which is based upon the completeness-assumption has so far been successful. This applies also to Bohm's theory of 1951 whose 'hidden' parameters are not independent of the parameters of quantum mechanics proper, but partly definable in terms of them (which is connected with the fact that statements about his 'Ψ-field' and about the trajectory of a particle moving under the influence of this field, are not logically independent). Bohm's theory only refutes von Neumann's contention (326) to have shown that even the existence of *untestable* deterministic parameters contradicts QM in its present form.

Measurement. Von Neumann's theory of measurement can be regarded as an attempt at a rational explanation of the old idea of 'quantum-jumps' (p. 218 n.). It rests upon the assumption that the state of a system may undergo two different kinds of change, viz. changes which are continuous, reversible, and in accordance with the equations of motion—those changes happen as long as the system is not observed, however strong its interaction with other systems may be ; and changes which are discontinuous, irreversible, not in accordance with the equations of motion and which happen as the result of a measurement. Two arguments are presented for this assumption, an inductive argument, trying to relate the existence of the discontinuous jumps to experience (pp. 212 sqq.), and a consistency-argument which shows how they can be fitted into the theory without leading to contradictions.

The inductive argument is invalid (it is based upon an analysis of the Compton-effect which takes into consideration only those features of the phenomenon which can be understood on a classical basis). The consistency-argument is sound but it leads to a theory of measurement which is incomplete (it omits the fact that the measuring-device is macroscopic and that the observer can only ascertain its classical properties) and gives rise to various paradoxes (cf. my account in *Zs. Physik*, **148**, 1957): von Neumann has not succeeded in justifying the idea of quantum-jumps on the basis of the theory.

The remainder of the book deals with the theory of radiation, with thermodynamical questions (here Szilard's highly original ideas are used and extended) as well as with more specific problems of interpretation.

Trying to evaluate the book we may say that its main merit lies in the fact that (1) it gives us a 'minimum presentation' of quantum mechanics, i.e. a presentation which is based upon as few assumptions as possible; (2) it gives us a presentation of the theory that is formally much more satisfactory than many of the preceding expositions; (3) it tries to show how much of the Copenhagen-ontology can be justified on this basis, and thus be shown to be part of physics rather than part of philosophy; and (4) it contains valuable contributions to many special problems. In this way the book has greatly contributed to clarifying the foundations of the theory. However it contains a theory of measurement which is incomplete and leads to paradoxical consequences. This is the more surprising as all the elements needed for completing the theory had been developed by von Neumann himself already in 1929, in his paper on the quantum-mechanical H-theorem and ergodic theorem (*Zs. Phys.* **57**), as well as in chapter V, section 4 of his book. (It was not before 1952 that these elements were used for attempting a complete theory of measurement.) Secondly the arguments which are used in order to carry out the programme referred to under (3) are by no means as straightforward as they seem to be at first sight. Reading the book one gets the impression that here *proofs* are presented to the effect that one cannot escape adopting the Copenhagen-interpretation once one has chosen to adopt quantum mechanics. This has led to the myth that the Copenhagen-interpretation has a firm 'mathematical' backing while all that could be shown was that it is compatible with QM and must therefore be defended by independent arguments (in which some of von Neumann's theorems do play an important rôle).[1] In this way von Neumann's book has created a good deal of confusion among physicists (cf. e.g. de Broglie's statement in the Introduction to *La Physique quantique restera-t-elle indéterministe ?*) and, naturally, among philosophers.

A final remark about the formalism used by von Neumann (description of states in terms of statistical operators). This formalism has hardly ever been used in philosophical discussions of the foundations of the theory. Such discussions are either based on the formalism of wave-mechanics, or (to a lesser degree) upon the formalism of matrix mechanics. The reason for this procedure is to be found in the fact that wave-mechanics, for example, has a particularly classical appeal which makes it more plausible because it suggests many seductive but often mistaken analogies. The many mistakes which philosophers have made when analysing the *content* of quantum mechanics on the basis of the formalism of wave-mechanics (as an example I may only mention Nagel's and Northrop's contention that quantum mechanics does not violate determinism as the wave-function obeys a deterministic equation) indicate clearly that a less prejudiced formulation of the theory is needed. Von Neumann's book contains such a formulation which should be known by everybody who is interested in the foundations of quantum mechanics.

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[1] It ought to be mentioned that Bohr himself did not commit this mistake.

P. K. Feyerabend

The Physical World of the Greeks. By S. Sambursky. Translated from the Hebrew by Merton Dagut.
Routledge and Kegan Paul, London, 1956. Pp. x + 255. 25s.

Western civilisation is distinguished from all other civilisations, past or present, by the fact that it has science. Religion is an ingredient of all civilisations. The arts and crafts have reached as high, if not a higher, level in the East as in the West. And basic technological discoveries like paper, gun powder, and the compass have in fact come to us from China. But science is a unique feature in our civilisation ; and we owe it to the Greeks.

Certainly modern science differs very much from the Greek enquiry into nature, both in content and in method. But if we want to understand our science we must know its origin and trace its development. The residue of past thought and especially of past error clings to the most modern concept, though it may be hidden and unnoticed underneath the heap of striking new facts and theories. Many philosophical problems that we find in science have their roots there—for example, that of determinism. Professor Sambursky's careful historical study will therefore be of interest to the scientist as well as to the philosopher who wishes to solve—or resolve—such problems. Starting with the views of the pre-Socratics, the author describes Pythagoreanism, the various cosmogonies and cosmologies, atomism, and the physics of Aristotle. An account of the Stoic school follows, and the author ends his story with Plutarch and Ptolemy whom he credits with having developed astrophysics from mathematical astronomy.

Though Professor Sambursky often points out the relation between the Greek concept and the corresponding one in modern physics, he generally refrains from giving an interpretation of the various views he describes ; he prefers to let the philosophers speak for themselves by means of suitably chosen quotations. The author has no specific thesis to contribute beyond showing that modern science arose from the Greek speculations. Thus the main value of this book for the philosopher of science is to provide source material.

This treatment is, I suppose, also due to the author's attitude (which he shares with many scientists) that he finds philosophy to be useless as far as research is concerned. Greek science began when 'the Milesian School opposed logos to mythos', while modern science started with Galileo who 'set science free from the swaddling-bands of the Church. . . .'. Yet Professor Sambursky finds that there is a fundamental difference between these two revolutions. The Ionians tied science to philosophy, while Galileo broke these bonds for good and all so that 'after Descartes and Leibniz there were no philosophers who contributed anything of importance to the exact sciences'.

But is the separation of science from philosophy really so complete? It is true that modern philosophers have been increasingly losing touch with science. Thus the scientists were forced to philosophise, and we need only think of Mach, Poincaré, and Einstein to see this. We cannot do without philosophy in the sense of subjecting our facts and theories to rational argument, so that we can separate more sharply reality from fantasy and distinguish physics from metaphysics. This is what philosophy has been in Greek times, and still is, all about—namely, to argue about general basic conceptions in a rational, and even logical, manner. Without such 'rationalisation of experience', if carried out properly with due respect for reality, there is no science. For no theory, however well established, is completely finished and free from error; Newtonian mechanics and Einstein's critique of the concepts of absolute space, time, etc. are examples. Science arose exactly because the Greeks were able, for a very short time, to conquer their fantasies and to reach a level of rational thought which other peoples were unable to attain.

The task of drawing a boundary around the domain of science and of weeding it out is always with us. This rational reconstruction of our experiences can be carried out by the philosopher only when he is firmly based on science; otherwise it becomes mere rationalisation, that is, system building for its own sake and, thus, a denial of external reality. The story of the Greek origin of physics as told in this book will help us doing the task and therefore contribute to our understanding of modern science.

E. H. Hutten

Whitehead's Philosophical Development. A Critical History of the Background of *Process and Reality*. By Nathaniel Lawrence.
University of California Press, 1956. P. xxi + 370. 37s. 6d.

The works of A. N. Whitehead are classifiable as falling within three periods. First, there is the mathematical period beginning with *Universal Algebra* (1898) and ending with *Principia Mathematica* (1910). This was followed by a methodological period, dealing with concepts of nature and scientific knowledge. Finally, we have the metaphysical period, of considerable range. Mr Lawrence begins his study with the second period, and devotes the first two parts of his book to it. Part III deals with what the author calls ' the transition period ' and is chiefly concerned with *Science and the Modern World*, *Religion in the Making*, and *Symbolism*. Part I is largely devoted to Whitehead's treatment of the epistemological problems which arise in connection with his attitude towards nature in his early methodological works ; his doctrine of events and objects and the method of extensive abstraction are also discussed at length. Part II covers much the same ground as Part I as far as topics are concerned but deals with the changes that had occurred in Whitehead's thought by the time *The Principle of Relativity* came to be written. Part III struggles with all the difficulties which accumulate as soon as we leave the province of methodology. As the author observes : ' The task of examining Whitehead's philosophy in the transitional period of his development is vastly different from that of analysing the early scientific period '. Whitehead now presses the topics of mind and value into the field of his investigation, thereby laying the groundwork for the vast synthesis of *Process and Reality*. The difficulties are increased by the fact that, as Mr Lawrence expresses it, ' there is a disappointing lack of example in all of Whitehead's works,' and also because ' Whitehead's use of language is occasionally difficult because of its novelty. It is also sometimes misleading because of its instability.' The book is commendably free from misprints, although there seems to be one on page 174, where we read ' an adjective of the event which it gratifies ' when what is intended seems to be ' qualifies '. The serious student and admirer of Whitehead should find much to interest him in this book. As Mr Stephen Pepper writes in the Foreword : ' Only those who have struggled over Whitehead's writings can fully appreciate the amount of clarification Lawrence has brought to an understanding of Whitehead.'

In short, Mr Lawrence undertook a task, which though limited, was formidable enough, and carried it out commendably.

J. H. WOODGER

Le Rôle médiateur de la logique. By H. Meyer.
Assen-Paris, 1956. Pp. 240.

THIS is a metatheoretical enquiry into the rôle of logic in mathematics and in natural sciences. The principal thesis of the author is that in classical mathematics the task of logic consists in establishing a calculus in which the variables take their values from among the properties and relations characteristic of sets of elements. In the natural sciences, on the other hand, logic is supposed to provide mediation between the ideas, however vague, which we entertain in relation to our observations, and the deductive system which we endeavour to construct. In the author's belief logic became a separate discipline as soon as methods had been discovered of manipulating symbols without any reference to their interpretation. Logic in this sense was not known to Aristotle but the Stoics, so the author claims, can rightly be regarded as the precursors of the formalist method. This method, he continues, was rediscovered by Leibniz and, since the middle of the nineteenth century, has been gradually accepted by logicians and mathematicians as the only satisfactory means of establishing the foundations of mathematics and theoretical physics. For the author, logic is a calculus of operators, and the conception of such a calculus involves, according to him, the following presuppositions. First, there have to be symbols applicable to a concrete and comprehensible domain. Secondly, the symbols have to be detached from their intuitive application and subjected to a calculus whose rules satisfy the requirements of symbolic manipulations. This is the phase of the calculus of operators proper. Thirdly, the rules of the calculus should be prescribed in such a manner as to guarantee that the results arrived at within the calculus shall be applicable to the domain from which the operators were originally detached. In order to justify this conception of logic the author gives a review of a surprisingly large variety of topics. Among many other things he discusses the concept of order and that of number, which—in a way reminiscent of Kant—he derives from our experience of succession in time. He presents the formalist and the intuitionist point of view in the philosophy of mathematics, talks about the Aristotelian syllogistic and about the logic of the Stoics, compares the Cartesian conception of the deductive method with that put forward by Leibniz, gives an account of the principle of complementarity, examines the rôle of logic in quantum physics, passes on to the problems of infinity and existence in mathematics, and ends by offering some comments on the semantics of formalised languages.

For the most part the book has an expository character with more attention paid to scope than to detail. As a result the reader is likely to find it difficult to follow the line of argument and may be at loss to see any connection between the main thesis of the author and the various topics that he touches upon in the course of the discussion. It has become rather fashionable to regard logic as a sort of computing gadget, and the activity of a logician is often reduced to the display of a skill in manipulating symbols. The author seems to favour this view. But his contention that logic is merely a calculus of operators might have commanded greater sympathy, had he been able to offer a convincing criticism of those antiformalist logicians (Frege and Leśniewski in the first place) who prefer to regard logic as a body of propositions which, in the most universal terms, describe reality.

Czesław Lejewski

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P. Welsh, ' On the Nature of Inference ', *The Philosophical Review*, 1957, **64,** 509-524

MINNESOTA CENTER CONFERENCE

A CONFERENCE, sponsored by the National Science Foundation, was conducted at the Minnesota Center for Philosophy of Science from 12th August to 14th September 1957. The participants were: H. Gavin Alexander, Eva Cassirer, H. Feigl (Director of the Center), P. Feyerabend, C. G. Hempel, G. Maxwell, H. Mehlberg, E. Nagel, H. Putnam, W. Rozeboom, M. Scriven, and W. Sellars. Daily group discussions and essays, circulated as memoranda, treated, extensively and in detail, the logical and philosophical issues of quantum mechanics, in particular, and of scientific theories, in general. Basic philosophical issues, such as the tenability of the analytic-synthetic and of the observational-theoretical dichotomies arose in connection with these issues of scientific methodology, and these were also discussed at some length. One of the products of the conference will be the third volume of *Minnesota Studies in the Philosophy of Science* (tentatively subtitled, ' The Philosophical Foundations of Physics '), which will appear in early 1959 and to which most of those who participated in the conference, as well as A. Grünbaum and possibly others, will contribute original essays and discussion remarks.